The Royal Aeronautical Society

(FOUNDED 1866)

with which is incorporated The Institution of Aeronautical Engineers

No. 259. JULY, 1932. Vol. XXXVI.

PROCEEDINGS

First Meeting, Second Half, 67th Session

The First Meeting of the Second Half of the 67th Session of the Royal Aeronautical Society was held in the Lecture Hall of the Royal Society of Arts, 18, John Street, Adelphi, London, S.W.1, on Thursday, January 14th, 1932, when a paper on "Interference," by Mr. E. Ower, was read and discussed. The President of the Society (Mr. C. R. Fairey) was in the chair.

The President: Aviation had always suffered from interference. There were some who said that it had suffered from Government interference, and others who held that but for that interference it would not exist; he believed the latter were right. But certainly within the past ten years they had accumulated a vast mass of knowledge on the subject of resistance in bodies travelling in a fluid, to such an extent that the modern aeronautical engineer could gauge very accurately—indeed, he could almost guess—the resistance of a given body. They knew very little, however, of the mutual resistance of bodies and the effect of one upon another, but, having glanced through Mr. Ower's paper, he could assure those attending the meeting that they would learn something about that subject. There was no better exponent than Mr. Ower. He had been on the staff of the National Physical Laboratory for more than twelve years, he was the author of a work on air flow, and had devoted many years to the exact study of this subject. The paper he had prepared for presentation to the meeting was most interesting.

INTERFERENCE

BY

E. OWER

§1. Introduction

1.1. Second in importance only to the problem of increasing the safety and reliability of aeroplanes is the question of reducing their head resistance. If aviation was to take its place as an established means of commercial transport the first essential was obviously to make it reasonably safe and certain, and the main energies of investigators and designers immediately after the War were directed to this object. Economy of operation, of which the reduction of head resistance is one of the major factors, was, by comparison, somewhat neglected. But in the last three or four years, with the safety and reliability problems essentially solved, more attention has been devoted to the reduction of drag.

1.2. In reviewing the progress that has been made, and the possibilities of further improvement, it is helpful to inquire what is the best we can hope for. We know that the generation of the lift required to carry weight in heavier-than-air aircraft is inseparably associated with a certain drag—the induced drag. The induced drag is the inevitable price we have to pay for transporting loads by

aeroplane. We know also that two other types of resistance oppose the motion of a body through air. The first is due to the friction between the air and parts of the body surface where the air in its immediate neighbourhood is streaming parallel to it,* and the second arises from turbulence set up when the direction of flow is sensibly not along the surface. Usually, drag due to turbulence of this nature is very much greater than that due to surface friction, but experience has shown that it is possible, at all events in simple cases, to suppress this turbulence completely by suitable shaping of the body; the flow everywhere at the body surface is then virtually parallel to it, and the drag, if the body is not lifting, is almost entirely due to surface friction.

1.3. We therefore postulate as our ideal aeroplane one whose total drag does not exceed the sum of the two unavoidable components—the induced drag and the surface friction—and a measure of the perfection of any design is the closeness with which it approaches this ideal. This point of view is due to Professor B. M. Jones, who put it before this Society in his illuminating lecture on " The Streamline Aeroplane " in January, 1928.[1] It may be recollected that Professor Jones's calculations indicated that many commercial aeroplanes of that date, which were not very different from those of to-day, consumed from one-and-a-half to three times as much power as the ideal aeroplane of the same induced drag.

1.4. It is apparent, therefore, that there is considerable room for improvement in design, quite apart from any decrease that may be effected in the induced drag for a given lift. Aerofoil theory provides a proved and accurate method of estimating the reductions in this direction that would result from such expedients as increased aspect ratio and modified plan form and shape of wings. Unfortunately, design and structural considerations limit the reduction of induced drag that can be effected. In fact, without profound departures from existing practice there appears to be little prospect of appreciable gain in this respect.

1.5. When, however, we come to consider that part of the drag independent of the lift, the situation appears more favourable. Since we have learnt by experience how to design " streamline " bodies, that is, bodies whose drag is mainly surface friction, there would seem to be no theoretical reason why it should be impossible to construct combinations of these bodies, to form a complete aeroplane, such that the drag of the entire structure is also mainly due to surface friction. But the problem is complicated by the phenomenon known as " interference," as a result of which the various parts of the structure are usually found to react upon one another aerodynamically. For example, two bodies each of which by itself may have approximately " streamline drag," may, when close together, have a combined drag differing appreciably from the total of the separate drags. For streamline bodies this difference is generally an excess, that is, there is an added drag due to interference, but it is not inconceivable that a streamline combination could be found in which the interference drag would be negative; negative interference has definitely been obtained when certain excrescences have been attached to streamline bodies (see below). It will be shown later that the interference effect may be of two kinds, one in which there are in the combination sources of turbulence which are non-existent in the undisturbed flow past the components, and another in which the pressure and velocity distributions round the components are changed. The former type must always result in increased drag, but the latter may have an opposite effect. In the case of a wing, interference of the latter type will also change the lift and the induced drag.

1.6. A noteworthy feature of the interference effect is that it tends to become more marked as the shapes of the individual parts of aeroplanes improve aerodynamically. This is illustrated by the fact that in many of the early aeroplanes

* Accompanying the frictional drag there is in general a " form drag " (see §3.2), but this is usually small, and we may neglect it for the moment.

interference appears to have been negligibly small. Thus, in the Annual Technical Report of the Advisory Committee for Aeronautics for 1912-13, page 185, an estimate of the drag of a complete model of the B.E.2e aeroplane from the separate drags of its component parts was found to agree very closely with the measured resistance of the whole model. This comparison was only made at one incidence, but similar (unpublished) comparisons were carried out at a later date on another model over a wide range of incidence. Again good agreement was observed. It was concluded from such experiments that the resistance of an aeroplane could be deduced very closely by summation of the drags of its component parts measured separately. But events proved this conclusion to be premature, for as design improved it was found not to be generally true. In the light of present knowledge there is little doubt that interference was absent in these early machines partly because the design was less compact than it usually is at present, but mainly because the shapes of the parts, and in particular that of the body, were aerodynamically so bad that the flow could not be made appreciably worse by interference.

1.7. The study of interference problems is confused by the great variety possible in the shapes and dispositions of the component parts of aeroplanes, and it is not easy to decide which line of attack is likely to yield results of the greatest value to designers. On the one hand there is the possibility of investigating in the wind tunnel the effect on the drag of a complete model aeroplane of successive modifications having reduction of drag as their object. This method is efficacious in reducing the drag of particular designs of aircraft—it has, for example, been successfully used for models of the 1927 Schneider Trophy racing seaplanes, which, in their final form, proved to have practically no interference between their component parts[2]—but it is not the one calculated to throw much light on the nature of interference, and so to give results of the widest possible application. More success in this direction is likely to come from what I may term a synthetic research which starts with the simplest kind of problems and gradually approaches the more complicated cases. This method has been followed in the researches conducted at the National Physical Laboratory during the last three years, a discussion of which forms the main substance of this paper.

1.8. The starting point in these researches was an examination of the flow past streamline bodies and of the extent to which it was disturbed by various types of excrescences. At a later stage wings were added, and the effect of nacelle-wing interference was also studied. A further step, which will be begun as soon as possible, will be the addition of the airscrew to body-wing combinations. In planning the researches shapes were purposely chosen in which the interference effects under examination were likely to be exaggerated. Although the resulting combinations only distantly resembled real aircraft in their proportions and dispositions, it was thought that work with them would be more likely to make generalisation possible than work with models more nearly representative of current practice. For the same reason relative positions of the components were sometimes chosen more with reference to the type of interference obtained than on the basis of their resemblance to combinations found in actual aeroplanes.

§2. Scope of the Paper

2.1. In this paper I shall discuss various experiments carried out in this country and elsewhere, and although we shall find that many factors play a part in interference phenomena, I shall endeavour to show that the evidence is consistent in indicating a few outstanding principles of general application to different branches of aeroplane design. I shall start with the simple streamline body, giving a brief *résumé* of the characteristics of the flow past it which are now regarded as established. Next I shall consider how these characteristics are changed by attaching certain types of excrescence to the body and how the body affects the excrescences themselves. The particular case when these

excrescences form the wings or parts of wings is one of special interest to aeroplane designers, and this will be discussed at some length. Nacelle-wing interference will then be treated, and this will be followed by a brief discussion of matters such as the effect of wing tanks, of cut-away centre sections, and of struts. Finally, some points arising in the carrying out of interference experiments in the wind tunnel will be considered.

2.2. With few exceptions, the effect of the airscrew slipstream is not taken into account, mainly because, apart from the question of airscrew-body interference which formed the subject of a lecture by Lock to this Society,* no experimental data exist. I may say here, however, that it is the intention, when an opportunity occurs, to do some work at the National Physical Laboratory to investigate how, if at all, the slipstream modifies certain types of interference observed in its absence. I consider it unlikely, however, that any of the factors which I shall indicate as promoting bad interference drag in the absence of the slipstream will be so modified by the slipstream as to cease to be detrimental.

§3. The Characteristics of Streamline Bodies

3.1. I propose to evade criticism of any attempt to give a rigid scientific definition of the term " streamline " body by calling it simply a " body whose drag is mainly due to surface friction." This rather flexible definition is practically convenient and accords, I believe, with the manner in which the term is generally interpreted by designers. Some years ago the study of the characteristics of streamline bodies would have had little bearing on practical aeroplane construction, but now I think everyone will admit that design is tending more and more towards better streamlining of the individual parts of aeroplanes, and consequently the properties of streamline shapes are no longer of merely academic interest to the aeroplane constructor. I offer no apology, therefore, for giving the following brief summary of these properties. In this field we have again to thank Professor Jones for providing us with a rational foundation on which to erect an ordered structure of our knowledge.

3.2. The essential feature of a streamline body is that near the surface the relative air flow follows the contours throughout the entire length from nose to tail except, sometimes, for a very small region at the extreme tail. Exploration of the velocity distribution along a normal to the surface at any point shows that the velocity rises from zero at the surface to a certain value which, over the front portion of the body, could be calculated with good accuracy from inviscid fluid motion theory if the shape of the body were mathematically defined. This velocity gradient occurs in a comparatively thin layer of air, known as the boundary layer, whose thickness depends both on the speed and the distance of the point from the nose of the body. It is the shearing that takes place within this layer that is responsible for the surface friction drag set up on the body. In addition to the surface friction there is, in general, a drag due to the downwind component of the integrated normal pressures acting over the whole body surface. Usually in a well designed body this component is small, cases being known in which it is actually zero within the limits of accuracy with which it can be measured, which, incidentally, are comparatively poor unless great care is taken and pressures are observed at a very large number of points on the surface. The drag due to this cause is called " pressure " or " form " drag.

3.3. Recent research[3, 4] has shown that the boundary layer for a streamline body resembles in character that for a flat plate. Amongst other features, the most important to note here is that two types of flow in the layer are possible; these are known as " laminar " and " turbulent " respectively. For our present purpose they may be distinguished by the broad statement that the laminar state is characterised by considerably greater steadiness within the layer than the

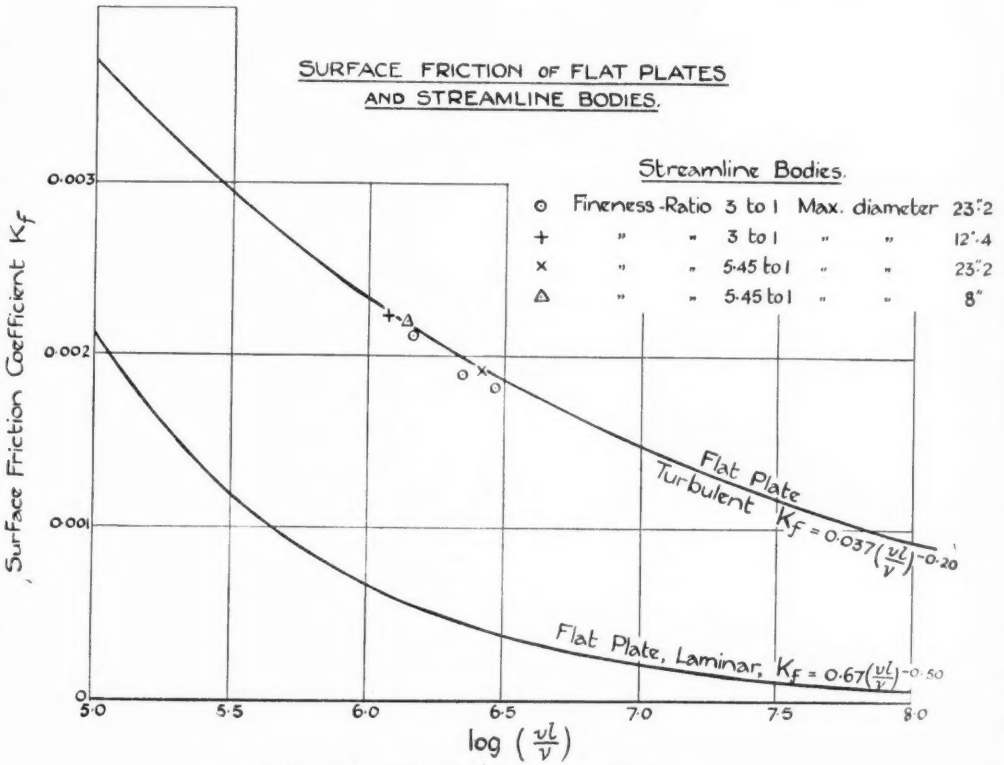
* August, 1929.

Surface Friction Coefficient K_f

turbulent state. The former type of flow appears to be stable only for comparatively low values of the Reynolds number vl/ν (v=relative air speed, l=distance from nose along axis, ν=coefficient of kinematic viscosity of air); at higher values it breaks down into the turbulent state. The critical Reynolds number at which the transition takes place lies roughly between 10^5 and 10^6, the actual value depending upon the degree of turbulence present in the air surrounding the body; the more turbulent the air the sooner the transition occurs.

3·4. A distinction must be drawn between a turbulent boundary layer and the type of turbulence outside the layer which has been already mentioned as being responsible for very high drag. A broad description of the latter type of turbulence is contained in the statement that it implies that the flow is no longer smoothly following the surface of the body but has broken away from it; eddies and reversal of the direction of flow at the surface are set up. On the other hand, although the boundary layer itself may be turbulent the flow still clings to the surface, otherwise there would be no true boundary layer in the accepted sense of the term.

3·5. The intensity of the surface friction depends on the type of flow in the boundary layer. The curves of Fig. 1 show the values which may now be regarded as established for smooth flat plates. It should be noted that, although the curves have been drawn to cover the same ranges of Reynolds number R,



$\rho v^2 K_f$ = Frictional drag per unit surface area.

FIG. 1.

each is only valid for that range of R in which the type of flow it represents can exist. Now the resemblance of the boundary layer of a streamline body to that of a flat plate is more than a mere qualitative similarity, for it appears from recent careful measurements[4, 5] that the intensity of surface friction for a streamline body with a turbulent boundary layer agrees closely in magnitude with the value at the same value of R taken from the turbulent curve of Fig. 1. The bodies used for this work are shown in outline in Fig. 2, and the observed values of the frictional drag per unit area of body surface are plotted in Fig. 1. The agreement is very good despite the fact that one of the bodies had a fineness ratio of only 3 to 1, with regions of relatively rapid curvature on its surface.

3.6. The large difference between the intensities of laminar and turbulent surface friction at high Reynolds numbers shown by the curves of Fig. 1 provides material for speculation. The average value of R for full-scale aeroplane bodies may be taken as 2.5×10^7, so that, if turbulent flow in the boundary layer sets in at $R = 10^6$, it follows that about 95 per cent. of the average aeroplane body has a turbulent boundary layer. Could we, therefore, devise means for inducing the laminar type of flow to persist at the Reynolds number of flight the frictional drag of a typical fuselage would be reduced to roughly one-ninth of its present value. So far there is no indication of how, or even whether, such a state of affairs can be promoted. On the other hand, there is no difficulty about inducing turbulence in the boundary layer at lower Reynolds numbers (above a certain low limit) than that at which it would start if left to itself, either by introducing disturbances into the general airstream or by attaching quite small excrescences to the body surface. This point is of practical importance in experimental work on the wind tunnel and will be discussed at a later stage in this paper.

§4. The Effect of Excrescences on the Characteristics of Streamline Bodies

4.1. We turn now to a consideration of the effect of certain excrescences on the drag of streamline bodies. Some interesting results have been obtained by attaching circular discs at various points along the surfaces of the two streamline bodies shown in Fig. 2. The bodies were 23.2in. maximum diameter and the discs, 4in. in diameter, were cut from 1/32in. tin plate. In each position they were attached with their planes normal to the local tangent to the body surface, the two discs being at opposite ends of a body diameter. Measurements were made of the combined drag of body and plates, of body drag and plate drag separately and of body drag with the plates held independently just clear of the body. The data enabled an analysis of the interference to be made and the results for the shorter of the two bodies are shown in Fig. 3. We see that, except with the discs attached at points within a small region near the nose of the body, the drag of the combination is everywhere greater than the sum of the free drags, the maximum excess—about 54 per cent.—occurring when the two plates are attached to the body roughly at the section of maximum diameter. We see also that the main effect is an increase in the drag of the body, the plate resistance being changed by a relatively much smaller amount.

4.2. At first sight it may be thought that the large increase in the drag of the body is due to the fact that the type of flow in the boundary layer is changed from laminar to turbulent by the disturbance in the lee of the discs, but closer examination shows that this explanation is not sufficient. The drag was augmented most when the plates were roughly 42in. behind the nose of the body, and other experiments showed that the boundary layer here, and, in fact, well forward of this section, was already turbulent in the absence of the discs. Further, when the plates were less than 8in. from the nose the body drag was actually reduced. A more satisfactory explanation of the observed facts would seem to be that the form drag of the body was changed by the presence of the

plates. One would expect a rise of pressure ahead of the discs and a fall behind them and it is conceivable that the resulting pressure distribution on the body might account for the observed changes of drag. During the course of some experiments with a smaller scale model of the same body the opportunity was taken of verifying this hypothesis. The distribution of pressure on the surface of the body was measured with discs of the same relative size as for the original experiments at positions corresponding to 5.2in. and 42in. from the nose on the large body. Fig. 4 shows the mean pressures around various sections of the body both with and without discs, and it will be seen that the expected pressure rise upstream and fall downstream of the plates are present. The low pressure loop, however, is predominant, and it is thus easy to see that, taken in conjunction with the shape of the body over which it is operative, it accounts for the type of drag change experienced.

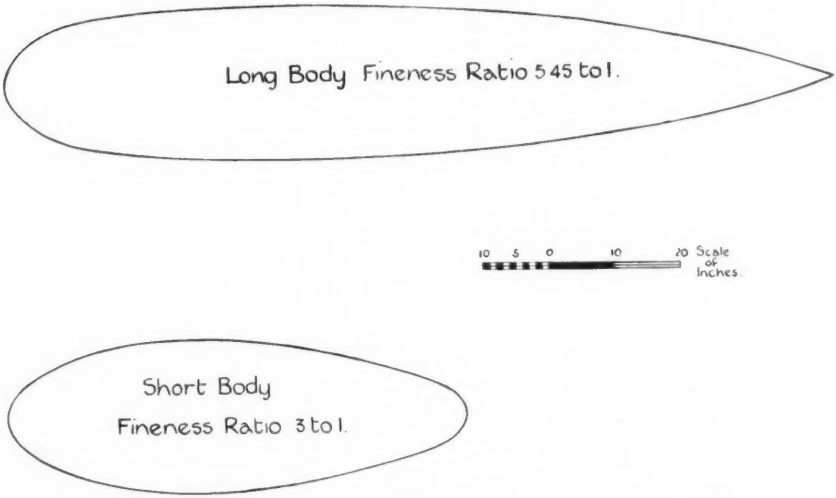


FIG. 2.

4.3. Integration of the pressure curves in the two cases indicates that the discs in the forward position should decrease the body drag by about 38 per cent., and in the rear position increase it by 105 per cent. The corresponding percentage changes obtained by actual force measurement were 21 per cent. reduction and 164 per cent. increase. The pressure distribution does not, therefore, account for the whole of the change; in both cases the actual body drag is higher than the pressure distribution indicates. With the discs in the forward position this is not difficult to understand, as the boundary layer was there laminar and the disturbance introduced probably caused it to become turbulent sooner than on the bare body. In the rear position, however, the indications from other experiments were that the boundary layer was already turbulent, and a detailed examination of the flow would have been necessary to find why the body drag was increased by more than the amount due to the changed pressure distribution. Possibly the plates influence the position of the transition from laminar to turbulent flow even when they are some distance downstream of its position in the undisturbed flow.

4.4. As an extension of these experiments similar work was carried out with a small streamline body of maximum diameter equal to the diameter of the discs.

The object of these tests was to investigate the effect on the interference of an excrescence of the same cross-sectional area as before, but of much better aerodynamic shape. The drag of this body at 60 ft./sec. wind speed was only 0.016lb. as compared with 0.442lb. for one circular disc and 0.540lb. for the

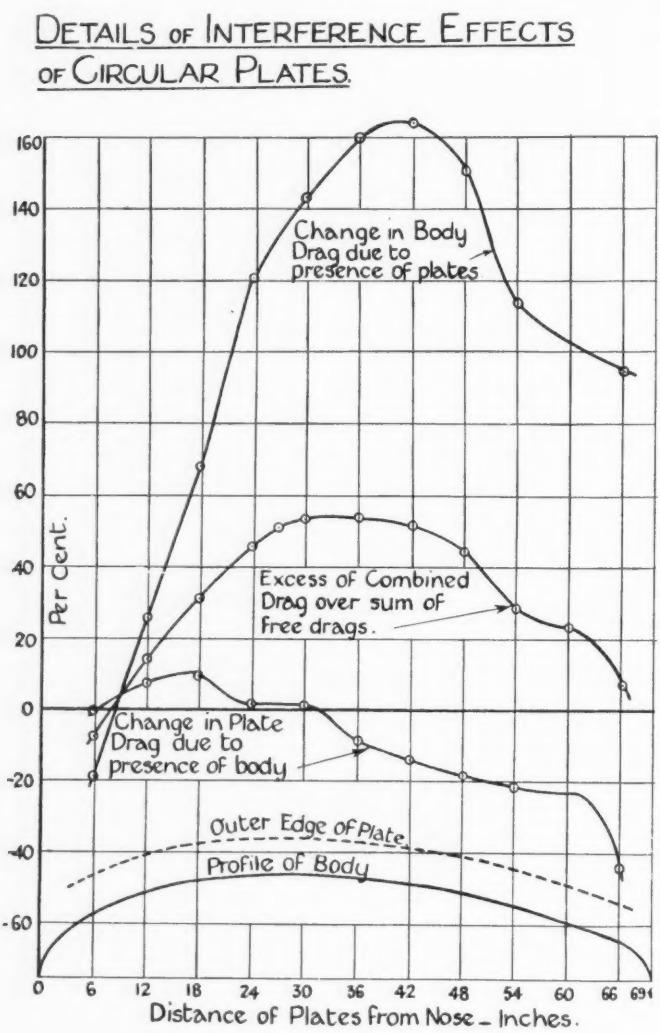


FIG. 3.

large body. It may be mentioned that in each position of the small body on the large one the axis of the former was arranged to be parallel to the local tangent of the latter at the point of nearest approach of the two surfaces. Fig. 5 shows the results obtained compared with those for one disc. As regards the

drag of the large body in the presence of the obstruction it will be seen that although the run of the curves is roughly similar, there is everywhere a substantial reduction when the obstruction is faired. (We may consider the small streamline body as a faired disc.) On the other hand, the percentage total interference for the combination is not so favourable to the faired obstruction; indeed, over an appreciable forward part of the large body, we have an example of the principle mentioned earlier in this paper, viz., that interference may be worse as the aerodynamic shape improves. To avoid misconception, it should, however, be stated that, although in certain positions of the obstruction the percentage change in drag was greater with the small streamline body, the overall drag of the combination was always less than with one circular plate on account of the very large difference in the free drags of these two interfering objects.

EFFECT OF DISCS ON PRESSURE DISTRIBUTION ON STREAMLINE BODY.

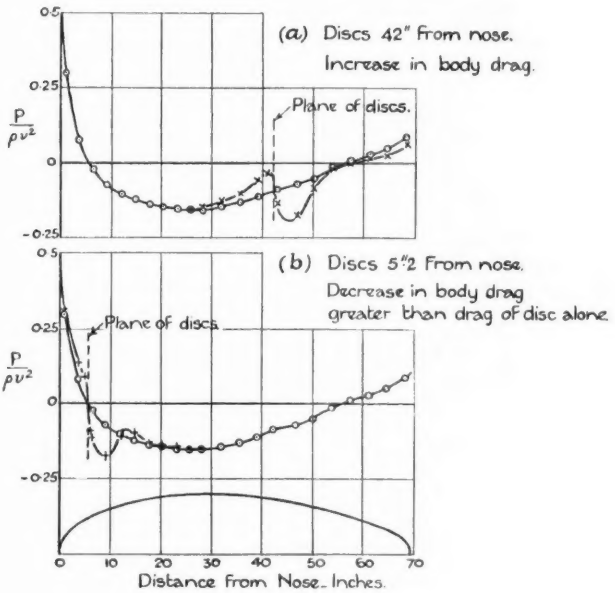


FIG. 4.

4·5. The nature of the interference observed with the small body appears definitely to differ from that experienced with the discs. Fig. 6 is an outline sketch of the large body with the small one attached at a typical position near the nose. From this we see that the observed effects would be explained qualitatively if a local acceleration in the air flow occurred near the point of contact. For then the pressure change accompanying this acceleration would have an upwind component acting on the surface of the large body whose drag would be accordingly reduced, and a downwind component on the small body, thereby increasing its drag. Fig. 5 shows the reduction of drag actually measured on the large body, and it may be mentioned that large increases of drag of the small body were duly observed. In the foremost position, for example, its drag

was increased to more than 23 times its drag in free air. This type of interference is an instance of what appears to be a general principle, other examples of which will be given later. It would seem to be due to the convergence of the surfaces forming the noses of the two bodies; the flow of air into this converging region causes the local acceleration postulated above, with the consequent reduction of pressure, as occurs, for example, in the entry to a Venturi tube. There would seem to be scope, by suitably inclining the surfaces

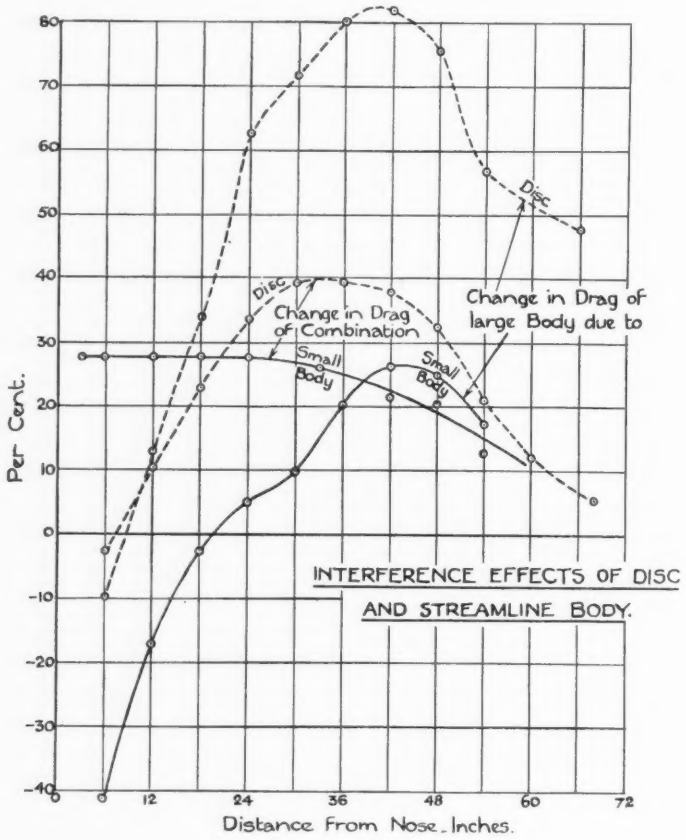


FIG. 5.

over which this reduced pressure acts, for decreasing the resulting drag components, but the possibilities in this direction will be limited by the necessity for avoiding turbulence after the air has passed the local convergence.

4.6. Two other types of excrescence were also tried on the same streamline bodies. These consisted of rings of sheet tin attached successively round circumferences of the body at various distances from the nose, with their inner edges in contact with the body surface, and also rings of thread, string, and rubber tubing, constituting a series of rings of circular cross-section varying from 0.01in. to 0.375in. diameter, similarly attached. Each tin-plate annulus

INTERFERENCE OF SMALL STREAMLINE BODY.

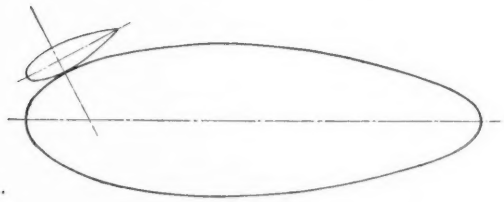


FIG. 6.

INTERFERENCE EFFECTS OF ANNULI AND DISCS.

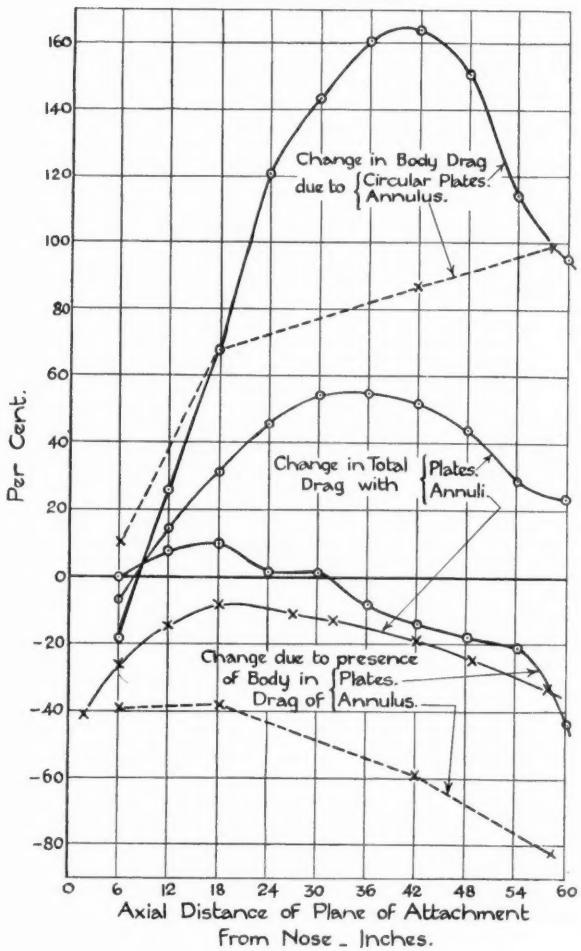


FIG. 7.

had an area equal to the total area of the two 4in. diameter circular discs previously used. The free drags of the annuli were measured separately, and the drag of the annuli when attached in four positions on the body was estimated from pressure measurements on the front and rear faces of the annuli.

4.7. An analysis of the results obtained with the annuli is given in Fig. 7. With regard firstly to the total interference effect, we see that for every annulus position the drag of the combination is less than the sum of the free drags, *i.e.*, the total interference drag is negative. Thus, although the annuli were of

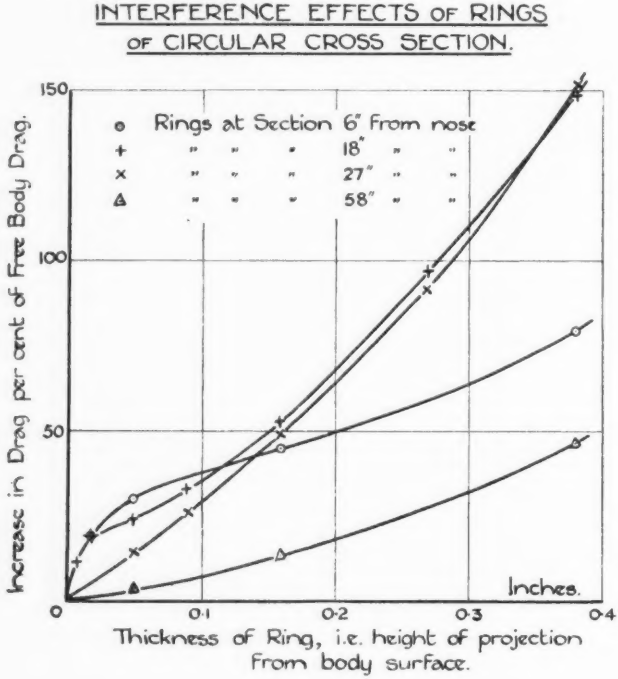


FIG. 8.

the same area as the two circular discs, and although their free drag was about 1.7 times* that of the two discs, the interference effect of the annuli was considerably less than that of the discs. This, no doubt, is partly due to the fact that an appreciable part of the annuli were in the retarded boundary layer of the body, for the annuli ranged from 0.33in. to 0.85in. in thickness, whereas the discs projected 4in. from the surface. The curve for the effect of the body on the annulus resistance (Fig. 7) shows up this point by the reduction in drag of the annuli. It is also probable, however, that the change in the pressure distribution on the body, which in the case of the discs was found to exercise so large an effect on the body drag, was less pronounced with the annuli.

4.8. Interesting also are the results obtained with the rings of circular cross-section. No analysis of the interference was attempted in this case, measurements being made only of the overall increase in drag due to the rings of progressively increasing thickness at four different transverse sections of the body.

* This is approximately the ratio of the drag coefficient of an annulus to that of a disc.

This increase in drag, expressed as a percentage of the free body drag, is plotted against ring thickness in Fig. 8, and it will be seen that, although the drag of the combinations always grows as the ring thickness becomes greater, the type of growth depends within certain limits upon the position of the rings on the body surface. Thus, for sections 6in. and 18in. from the nose the curve rises much more steeply at the origin than for the two sections further aft. That is to say, in the forward positions a ring of small thickness causes a much greater increase of drag than the same ring when attached further back. There seems little doubt that the explanation is that the boundary layer of the body was laminar at least up to the 18in. section and that the addition of a very fine thread was sufficient to introduce turbulence and so increase the surface friction drag of the body. Experiments of this kind would seem, therefore, to provide a means of detecting the position of the point where laminar flow in the boundary layer breaks down.

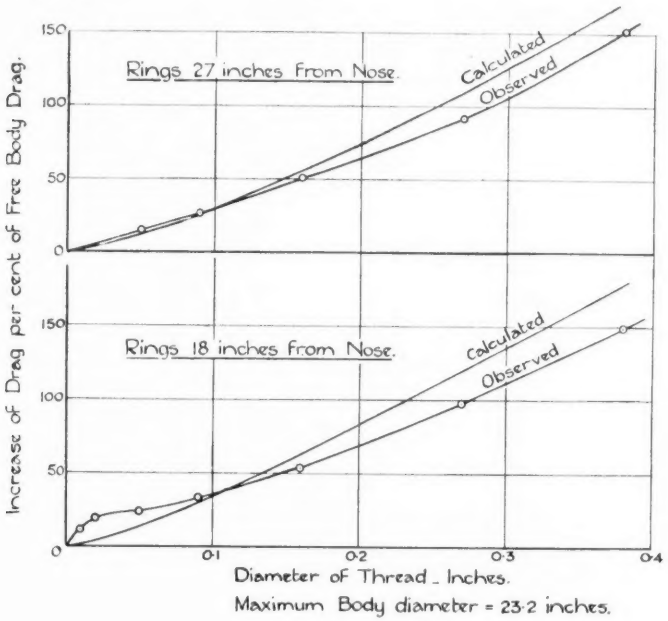


FIG. 9.

4.9. In Fig. 9 a comparison is given, for the 18in. and the 27in. sections, of the observed increase of drag with ring thickness, as plotted in Fig. 8, and that calculated on the assumption that the increase is due only to the drag of the ring in a turbulent boundary layer. In making the calculations the thickness of the layer and the velocity just outside it have been taken from measurements (not here reproduced, see reference 4) made on the body itself, and the velocity distribution in the boundary layer has been assumed to be given by von Kármán's formula, which has been established experimentally for flat plates by Burgers and Van der Hegge Zijnen:—

$$u/v = (y/\delta)^{1/7},$$

where u is the velocity at a distance y from the surface,
v is the velocity just outside the layer,
and δ is the thickness of the layer.

From this formula the mean air speed in which the ring was situated could be deduced, and hence its drag was calculated on the further assumption that its drag coefficient was the same as in free air.

4.10. Despite the numerous assumptions made it will be seen that, provided the ring thickness is not too large, there is reasonably good agreement between the calculated and the measured increase of drag, although the calculations take no account of the rapid increase of drag for very small ring thicknesses when the boundary layer is laminar to start with.

4.11. Some of the experiments with excrescences on the shorter of the two bodies shown in Fig. 2 were repeated with the longer body of fineness ratio 5·45 to 1. Fig. 10 shows the overall interference effects obtained with the 4in. circular discs and the annuli. Comparing these results with the corresponding data for the short body, we find that although the general type of interference is the same there is one new feature of some interest. This is the fact that, both as a percentage (as plotted) and in absolute value, the increase in overall drag with the circular discs attached is considerably less than that found with the shorter body over most of the length. This effect seems to be due to the fact that the maximum diameter of the long body occurs relatively much nearer the nose than it does in the short one, and also that thereafter the taper towards the

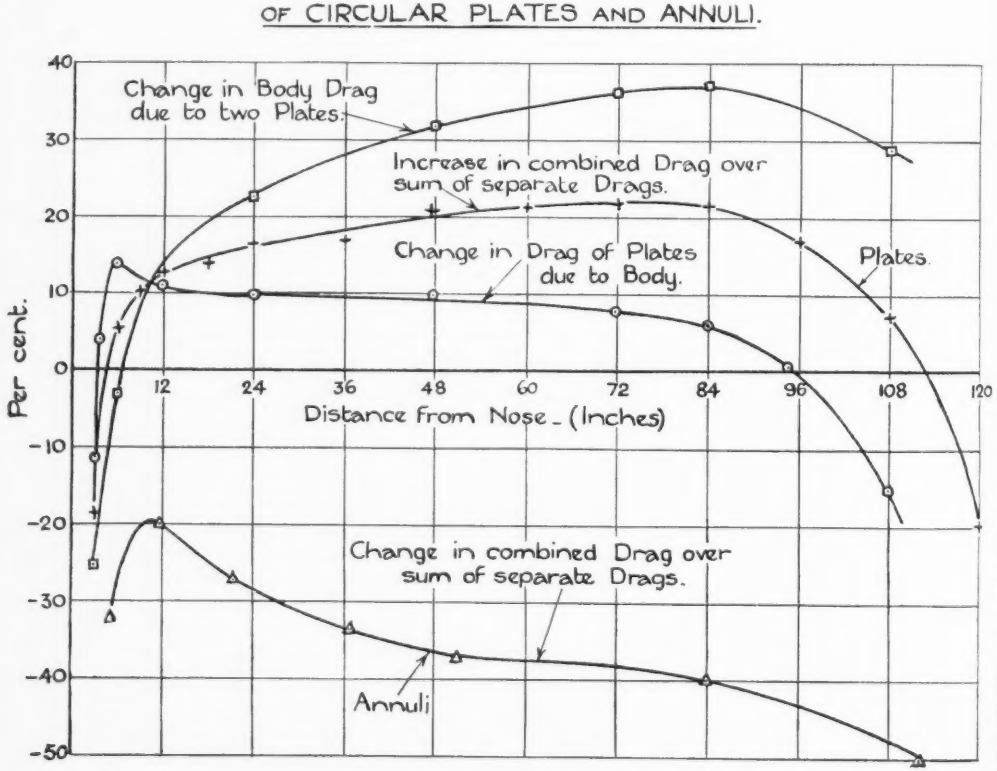


FIG. 10.

tail is more gradual. Hence the downwind component of the low pressure region behind the discs (see Fig. 4) would be expected to be smaller for the long body—on a cylinder with its axis along the wind, for example, the normal pressure would have no downwind component. As regards the annuli, the results again exhibit the feature of negative total interference found also on the short body.

4.12. Perhaps the most important conclusion to be drawn from these experiments with streamline bodies is that relating to the region of negative interference near the nose. It seems that, provided the boundary layer is turbulent, as it will generally be at full-scale Reynolds numbers, the addition of an ex-

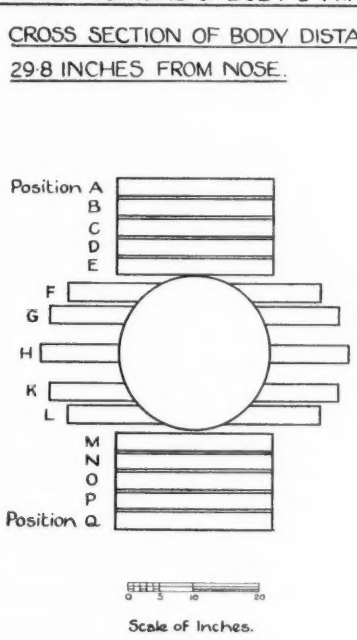


FIG. 11.

crescence near the nose of a streamline body is less detrimental to the drag than the addition of the same excrescence further aft. The extent of this favourable region depends upon the shape of the nose of the body; it is shorter for a bluff nose in which the maximum diameter is reached in a relatively short axial distance, than with a front portion of more gradually increasing diameter. Maximum interference is reached roughly in the neighbourhood of the maximum diameter or, more accurately perhaps, at a section where the diameter is not much less than the maximum diameter. Another important conclusion is that pressure changes play an important part in interference effects, and there may be some possibility of turning such changes to advantage by suitable arrangements of the interacting surfaces.

§5. BODY-WING INTERFERENCE

5.1. On this important phase of the subject data have been accumulated fairly plentifully during the last few years. Two important series of investigations have been reported, one from Canada[6] and one from Germany,[7] in each of

which overall measurements of lift and drag have been made in the wind tunnel for various wing-body combinations. In both cases the models have included wings of fairly large aspect ratio (5 or 6 to 1) and the scale of the experiments therefore has been rather on the small side. Work has also been in progress at the N.P.L., and here rather a different method of attack was adopted for the first experiments. On the assumption that most of the interference has its origin near the junction of the wings with the body, only wing roots were used, instead of the entire span, and as a result it was possible, by working with larger models, to investigate the effects in rather greater detail.

5.2. The bodies used in the N.P.L. work were those already described and sketched in Fig. 2, the more complete series of tests being performed with the shorter body of 3 to 1 fineness ratio. Provision was made on this body for attaching wing roots of R.A.F. 31 section and 2ft. chord at a number of positions, in each of which the wing chord was approximately bisected by the section of maximum diameter of the body, and the roots, symmetrically disposed on opposite sides, projected for a mean distance of half a chord from the body surface. At the junction with the body the roots were shaped to conform to the body surface. The outboard ends of the roots were finished square and dummy extensions were supported from the tunnel walls, butting, with a small gap, against the ends of the roots proper, in accordance with the normal manner of eliminating end effects. In this way the roots were made approximately equivalent to short lengths of wing having, apart from interference effects of the body, " infinite aspect ratio " characteristics. In addition to positions in which the span of the roots intersected the body a number of positions with the wings above and below the body was also investigated. In such cases the two roots were fastened together to form a single length of wing of 2 feet span, and were attached to the body by means of a single steel plate $\frac{1}{8}$in. thick and 6in. deep lying along the wind direction. Fig. 11 shows the complete range of positions tested.

5.3. In the first series of tests the body axis lay along the wind direction and the wings were at 0° incidence referred to the tangent chord (k_L for 6 to 1 aspect ratio=0.227). Fig. 12 summarises the results obtained. We see that the best position of the roots is approximately central (position H), the greatest lift and the least drag occurring there. As the roots are lowered with respect to the body the drag rises steeply to a maximum when the upper surface of the wing is tangential to the lower body surface. Accompanying this drag change there is a sharp fall of lift. Similar, but less pronounced, changes occur when the roots are raised above the median horizontal plane of the body, the upper tangential position being one of another drag maximum, but the minimum lift in this case occurs when there is a small gap between the lower surface of the wing and the top of the body. Expressing the interference drag, that is, the measured drag of the combination less the sum of the free drags of its parts, as a percentage of the sum of the free drags, we obtain values of 71.6 per cent., 46.5 per cent., and 26.2 per cent. for positions E, H and M respectively. It should be noted that the free drag of the roots was taken as the profile drag, since the induced drag for infinite aspect ratio is zero.

5.4. An interesting feature of the lift curve in Fig. 12 is the fact that the combined lift of body and wing roots is considerably greater, if the most favourable position is chosen, when the roots project from the body than when they are entirely above or below it. It seems, in fact, that the wings in the former position so modify the flow round the body as to cause the latter to contribute appreciably to the lift although its angle of pitch is zero. Confirmation of this view was obtained from measurements of the forces on the body alone with the wing roots detached from it but independently held in position H, just clear of the body. It was thus found that the lift on the body was 42½ per cent. of the lift of the combination with the roots in position H. It is noteworthy, also,

that the total measured lift agreed closely in magnitude with that calculated on the assumption that the wing roots were continuous through the body and formed part of a wing having infinite aspect ratio characteristics. Equally good agreement was obtained when the total lift was measured with longer lengths of roots

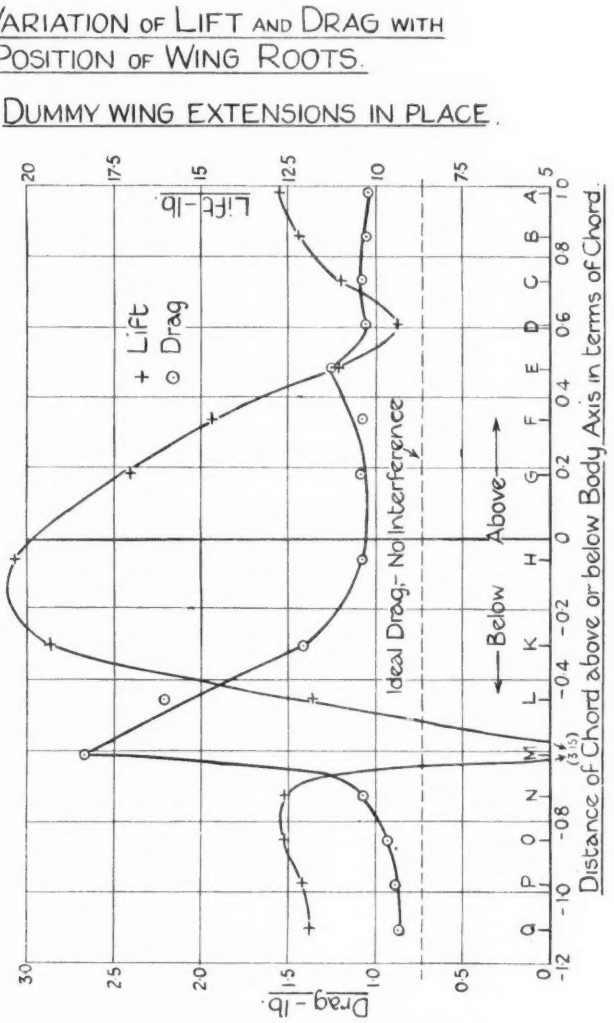

VARIATION OF LIFT AND DRAG WITH POSITION OF WING ROOTS.
DUMMY WING EXTENSIONS IN PLACE.
Lift - lb.
+ Lift
○ Drag
Ideal Drag - No Interference
Below
Above
Distance of Chord above or below Body Axis in terms of Chord.
Drag - lb.

FIG. 12.

attached to the body. There would thus seem to be grounds for the generalisation that in positions of small interference the lift on a body-wing combination in which the wings intersect the body is approximately equal to the lift that would be produced if the wings were continuous through the body and the latter were absent.

5.5. *Effect of Lift.*—All the experiments hitherto described were conducted at constant geometrical incidence of the wings and zero pitch of the body. It was thought desirable, however, to make a few additional tests to see how the interference depended on the lift. For this purpose attention was confined to positions H and L of the wings on the body as representing a good and a typically bad position, and lift was varied in two ways. Firstly, the angle of pitch θ of the body was changed, keeping constant the angle $\alpha-\theta$ between the wing chord and the body axis (α=angle of incidence of the roots); secondly, $\alpha-\theta$ was changed, keeping θ equal to zero. The experiments previously performed covered the case $\alpha=\theta=0°$; the additional cases now investigated were ($\alpha-\theta=5°$, $\theta=0°$), and the two zero lift cases ($\alpha-\theta=0°$, $\theta=\theta_1$) and ($\alpha-\theta=5°$, $\theta=\theta_2$), where θ_1 and θ_2 are the angles of pitch for which, with the wing chord respectively at 0° and 5° to the body axis, the lift of the body-wing combination vanishes. Further investigation of interference at zero lift was made for the case $\alpha=\theta=0°$ with wing roots of the symmetrical R.A.F. 30 section in positions H and L. These roots were of the same dimensions as the R.A.F. 31 roots in the corresponding positions.

5.6. Fig. 13 shows the results obtained. For the purpose of calculating the percentage interference drag, the drag of the combination in the absence of interference has been taken to be the drag of the body at the appropriate angle of pitch plus the profile drag of the length of wing roots used. As before, no allowance was made for induced drag for, in the absence of interference, the method of test with dummy wall extensions would have reduced it substantially to zero. We see from Fig. 13 that in position L the interference drag increases very rapidly with lift, while in position H there is also an increase, but much less pronounced. This seems to indicate that, in spite of the attempts to eliminate induced drag, the presence of the body, as might be expected, produces a departure from the uniform lift grading along the span of the roots and so gives rise to appreciable induced drag. Hence the increase in the drag when expressed as a percentage of the drag calculated on the assumption of zero induced drag. The results for zero lift, however, show that there is still perceptible interference when the induced drag is in fact zero. Although it might be argued that even with zero total lift on the combination there might yet have been an irregular lift grading with the R.A.F. 31 roots which would have produced induced drag, this can hardly have been the case with the R.A.F. 30 roots in position H. For then the combination was symmetrical about the horizontal plane containing the wind direction, so that neither wings nor body could have been lifting separately. Yet the interference drag was equal in magnitude to that found with the asymmetric R.A.F. 31 roots in the same position when the body was pitched so as to reduce the total lift to zero. It appears, then, that there is a residual interference drag at zero lift, which, even in asymmetric positions, cannot be ascribed to an induced drag effect.

5.7. *Effect of Fillets.*—A few experiments were made on the effect of fairing off the sharp angles at the junction of the roots with the body. It was realised that a wide choice of fairing was possible, and that it was desirable to adopt some scheme which would render comparable the results for different wing positions. Accordingly it was decided that the fairings should take the form of circular fillets whose surfaces would be generated by spheres of various diameters rolling in contact with the wing and body surfaces. The junctions of the roots and body were therefore filled with plasticine whose free surface was shaped, in accordance with this scheme, by means of circular or spherical tools. The radii of the fillets tested ranged from $\frac{1}{4}$in. to 3in., *i.e.*, from 0.0104 to 0.125 wing chords.

5.8. Figs. 14 and 15 show the effect of the fillets on drag and lift respectively, and the improvement they produced is very striking. In the most unfavourable position of the roots relative to the body, for example, a fillet of radius only 1/16 of the wing chord increased the lift by more than 100 per cent. and reduced

the drag by 20 per cent. In general, the improvement was greater the worse the position of the roots relative to the body, but it will be noticed from Fig. 14 that the largest improvement in drag was obtained not in the worst position (M) of the roots, but in the position above this, when the reduction due to a fillet of

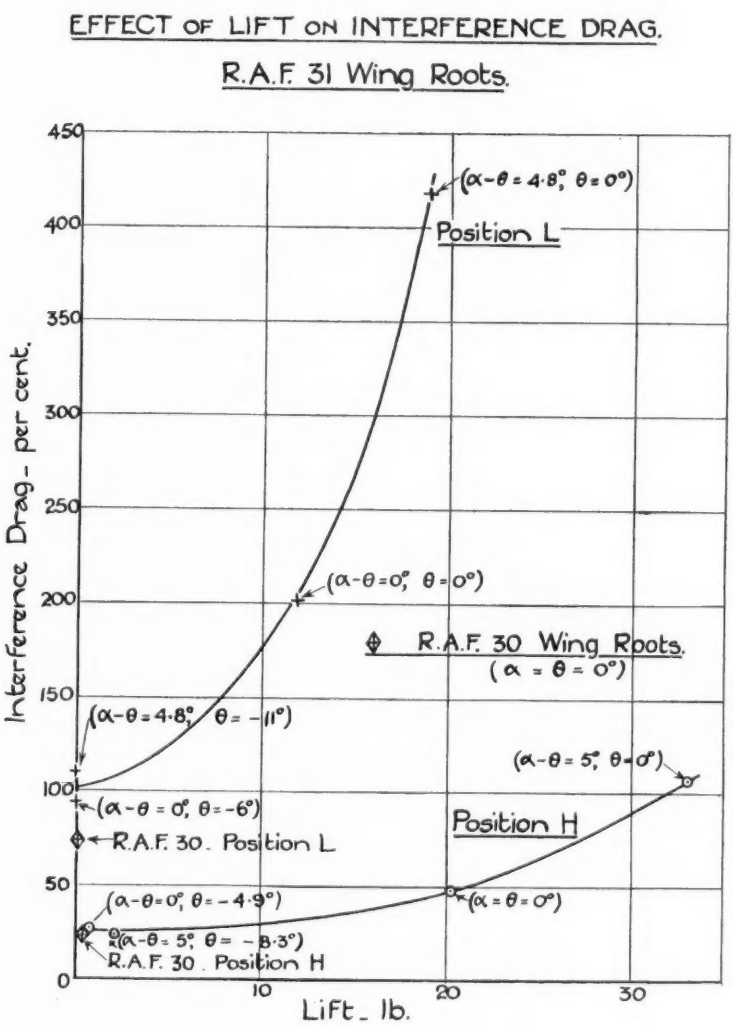


FIG. 13.

radius 1/16 chord was about 45 per cent. It is not improbable that this was due to the scheme of fairing adopted, which, in position M, led to a form having a very bluff rear portion that must have given rise to a local eddying wake. No object would, however, have been served by investigating this point, for the development of the best form of fairing in any particular case is a matter for

ad hoc tests. The results obtained are sufficiently indicative of the improvements to be expected from proper fairings.

5.9. A feature of interest in Figs. 14 and 15 is that, more especially in the case of drag, the improvement due to fairing as the radius of the fillet increases

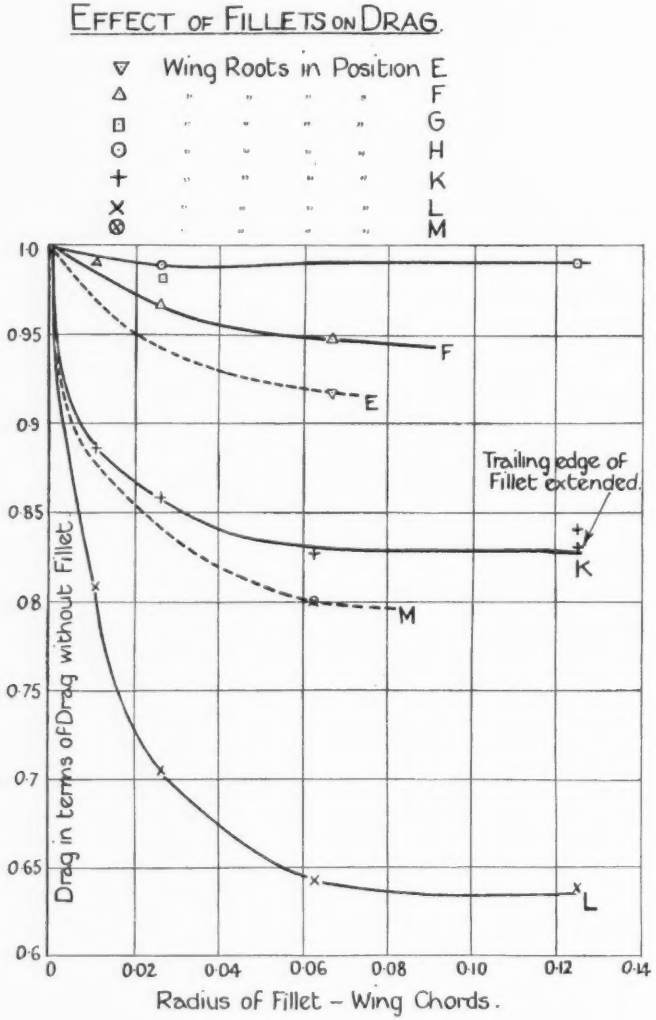


FIG. 14.

is very rapid at first and then falls off. Little is gained by increasing the radius of the fillet beyond 1/16 chord. We notice also that in positions E, F, and G, where the flow is relatively good, there appears to be a tendency for the fillets to decrease the lift slightly.

5.10. In one instance, with the roots in position K, the effect of a fillet on the upper surface of the wing only was compared with that due to fillets of the same radius on both surfaces. As was expected, practically the full effect was obtained with the upper fillet alone; the drag was, within the limits of experimental error, the same in both cases, whilst the lift was only about 2½ per cent. greater when the lower fillet was added.

Experiments with the Long Streamline Body

5.11. In order to obtain some data relative to the effect of body shape on body-wing interference a few experiments were made with the long streamline body of 5.45 to 1 fineness ratio. The work consisted mainly of a repetition of the investigation already made with the short body of the effect of variation of vertical positions of the wing roots at a constant axial distance from the nose such that the wind-chord of the roots was bisected by the plane of maximum diameter of the body. The method of procedure was exactly the same as that already described, the wing roots being again of R.A.F. 31 section, always at 0° incidence, with the body at zero pitch. The results may be considered in conjunction with Fig. 12, which relates to the tests with the short body. In brief, the character of the observed effects was the same in both cases, but the magnitude of the interference was less with the long body. With the latter, for example, the maximum interference drag, found, as before, for wing position M, was only about 33 per cent. of that for the short body, while in wing position H the corresponding figure was about 60 per cent. With symmetrical R.A.F. 30 roots at zero incidence in position H (zero lift) the interference drag was about 30 per cent. of the corresponding value with the short body.

Possible Explanation of the Interference Effect

5.12. The work so far considered may be open to criticism on the grounds that the combinations of wings and body chosen were of a kind that no designer would contemplate for a moment. But I submit that the results afford ample justification for the choice of exaggerated shapes, as they lead to important general conclusions, which might well have been missed had the experiments been done with combinations more closely resembling actual aeroplanes.

5.13. In all the combinations tested, the body surface was converging in the direction of flow aft of the mid-chord of the wing-roots. Hence, in order to follow both the wing and the body surfaces, the air stream behind the mid-chord would have to expand. Any such expansion would, of course, be accompanied by a positive pressure gradient in the direction of flow, the static pressure increasing at the expense of the speed. We have, in fact, conditions comparable with those existing in the outlet cone of a Venturi tube, and it is well known that if the angle of this cone is too great the stream cannot expand fast enough to fill the cone, and the flow detaches itself from the walls. The criterion of whether the cone will or will not run full is whether the kinetic energy in the boundary layer is sufficient to overcome the pressure gradient accompanying the expansion. With the body-wing combinations the conditions on the upper surface of the wing are more unfavourable still, on account of the steep positive pressure gradient that occurs on the upper surface of a wing behind the section of maximum suction; the flow has to overcome this gradient as well as that due to the geometrical divergence of wing and body surfaces. It is, therefore, not difficult to understand that a condition may well be reached when the kinetic energy of the boundary layer is insufficient to overcome the total pressure gradient. When this occurs the flow will detach itself from the upper wing surface, and also, probably, from the adjacent portion of the body surface, with a resulting increase in drag and decrease of lift. On the under surface of the wing the pressure gradient is of opposite sign, and so will, to some extent, assist the flow to adhere to the surfaces.

Also, in the cases tested, the divergence of the body surface from the lower wing surface was less than from the upper surface.

5.14. It appears, then, that the divergence of body and wing surfaces provides a possible explanation of the observed effects. Consideration of the geometry

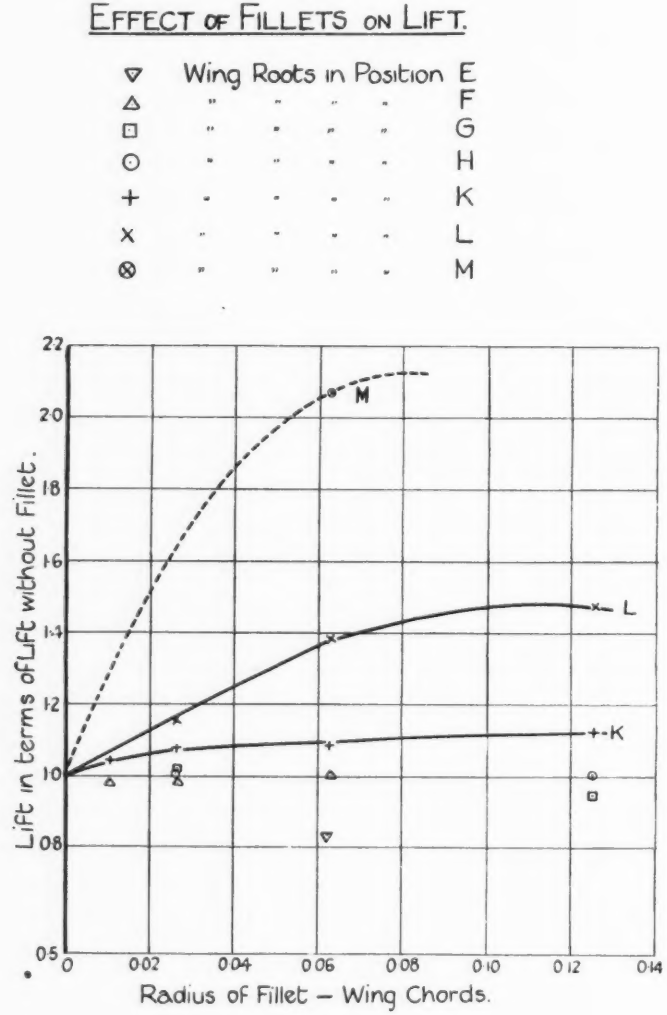


FIG. 15.

of the combinations furnishes confirmation of this hypothesis, for we find that the expansion over the rear half of the wing chord was greater for a given position of the wing below the horizontal median plane of the body than for the corresponding position above. On these grounds, therefore, we should expect to find

that positions below the centre of the body were worse than corresponding positions above, and also, since the divergence was greater the further the wings were above or below the median horizontal plane, that the interference became progressively worse as the roots receded from the central position H. Inspection of the curves of Fig. 12 shows that these anticipations accord with the results actually observed.

5.15. It also follows from the geometry of the combinations tested that the circular fillets on the upper surface of the wing to some extent reduced the amount by which the air had to expand to follow the surfaces. This, then, will account, qualitatively, for the beneficial effect of the fillets. They would have further improved the flow had they been made to increase in radius towards the trailing edge of the wing, so as further to reduce the expansion, and also had they been better shaped at the back in order to suppress abrupt changes of section of the structure in the region where the fillets terminated. Such fairings would operate in the sense that they retard the rate of expansion. It is in the boundary layer that any break-away that takes place has its origin, and since this layer is continually receiving fresh kinetic energy from the general airstream outside it as the flow proceeds along the surface, it is easy to understand that the rate of expansion is the important factor. The flow may be able to accommodate itself to a given expansion gradually, whereas a break-away might take place if the same expansion of the area to be filled occurred in a shorter length. The analogy of the Venturi tube is again useful here; by making the outlet cone of sufficiently gradual taper the flow may be expanded without loss of head (apart from wall friction) to almost any desired section.

5.16. Probably the fact that the divergence was less for positions of the wing roots above the centre of the body than for corresponding positions below, does not entirely account for the superiority of the former positions. When the wing is high on the body the part of the latter above the wing does not constitute so violent an obstruction to the flow over the upper surface of the wing (which is responsible for the major portion of the lift) as it does for low positions. Hence it is possible that the lift grading in high positions is less disturbed by the body. So many different effects play a part in the interference, however, that we cannot account closely for numerical differences which represent a final balance of all the factors. To name only one such factor as an example, the curvature of the stream due to the body, will, as pointed out by Parkin, alter the effective camber of the wing.

5.17. As additional support of the divergence hypothesis, we may adduce the results of the tests with the long body. In the positions in which the wing roots were tested, the divergence was considerably less than it was on the short body. This is clear from inspection of Fig. 2, where it is seen that the taper of the long body aft of the maximum diameter, where the roots were attached in both cases, is much more gradual than for the short body. And it has already been pointed out that the interference drag was substantially reduced.

5.18. Again, if the divergence of the wing and body surfaces is, in fact, largely responsible for the bad characteristics of the combination in which the wing roots are tangential to the lower surface of the body, we should expect an improvement to result from moving the roots forward axially with their upper surfaces still in contact with the body surface. Some experiments made to verify this suggestion provided strong evidence in its favour, but there were also signs that as the wing roots approached the nose of the body the interference extended further out along the span until it was clear that the lengths of the roots were insufficient. Additional experiments were therefore made with the 3 to 1 body and a complete wing. A Göttingen 420 aerofoil of 10-foot span and 2-foot chord was available and was used for these tests, which were made in the Duplex tunnel. Lift and drag of the combination were measured for four different axial positions of the wing relative to the body, in each of which the wing incidence

was changed to give results over a range of lift values. The body was at zero angle of pitch throughout, and the upper wing surface was always in contact with the bottom of the body. As will be seen from the following table, the interference drag decreased steadily as the wing was moved forward on the body. It should be stated that the percentage interference drag was found to be constant, in any one position, over the whole range of lift, the values given being the means of those measured. From the measured drag of the combination has been subtracted the sum of the free drags of the body and of the wing alone at the appropriate lift coefficient,* and this difference, expressed as a percentage of the sum of the free drags, has been called the percentage interference drag.

GÖTTINGEN 420 WING AND 3 TO 1 STREAMLINE BODY.

Axial distance in wing chords of leading edge of wing from nose of body.	Interference drag per cent.	Range of k_L
0.25 downstream	155	0.235-0.351
0	90	0.216-0.399
0.25 upstream	57	0.020-0.714
0.44 upstream	37	0.325-0.713

5.19. In addition to these tests, a few measurements were made, with the leading edge of the wing 0.42 and 0.25 chords upstream of the nose of the body, on the effect of moving the wing out from the body so as to leave a small clearance between its upper surface and the bottom of the body. In the former position it was found that the interference fell to zero when the gap was 0.125 chords, and in the latter position to 5 per cent. for a gap of 0.15 chords.

Foreign Work on Body-wing Interference

5.20. A discussion of the subject of body-wing interference would be incomplete without reference to the work of Parkin and Klein,[6] of Toronto University, and of Muttray[7] of Göttingen. An account of the former work has been published *in extenso* in the *Journal* of this Society,† and a brief summary of its salient features will suffice here. The tests were conducted on rather a small scale, the aerofoils used being only of 3in. chord and 18in. span, while the three bodies tested were only 2in. in their greatest width. However, the results were in close harmony with those of the large scale N.P.L. tests with the short span wing roots. The bad aerodynamic characteristics of the low wing monoplane were convincingly shown for all combinations of the three bodies and the thin, thick, and medium thickness wings. Moreover, in most cases the best characteristics over most of the flying range were shown by the combinations having the wings central on the body. As an exception to this generalisation, we may note Parkin's " fuselage B," sketched in Fig. 16, for which the wing position marked 3 gave the best characteristics over the whole flying range. The properties of the combination for the high wing position were quite markedly better than those for positions 1 and 2, which differed only slightly from each other. There seem to be indications here of a pronounced effect due to the shape of the nose of the fuselage.

5.21. As regards maximum lift, the wing on the top of the fuselage was the best of those combinations in which the wings were in contact with the body, while still higher maximum lift was usually obtained with a parasol arrangement in which the wing was above the body and clear of it by a small gap. The parasol arrangement was not, however, so good as regards minimum drag as the best position with the wings intersecting the body. The authors point out also that " fillets improve the characteristics of models that would otherwise have poor characteristics, especially when partial stall exists at low angles of incidence.

* The wing data had been previously measured in connection with a different research.
† January, 1930.

If, however, the model already has good characteristics, fillets will have practically no effect." Biplane models were also tested, in reference to which I quote the following conclusion: "With fuselage A (streamline fuselage), the biplane characteristics are midway between the characteristics for the two corresponding monoplane arrangements, but with fuselages B and C* the biplane drag for a given lift is definitely lower than for any of the monoplane arrangements. Where the fuselage does not fill the gap between the wings it appears best to place the lower wing at the bottom of the fuselage and the upper wing parasol." With reference to the last sentence it should be pointed out that bodies B and C had flat vertical sides and were parallel-walled in the positions occupied by the wings. Hence the low wing position was not nearly as bad as it would be on a tapering body of oval or circular cross section. It is doubtful whether the best biplane combination in such a case would be that recommended by the authors.

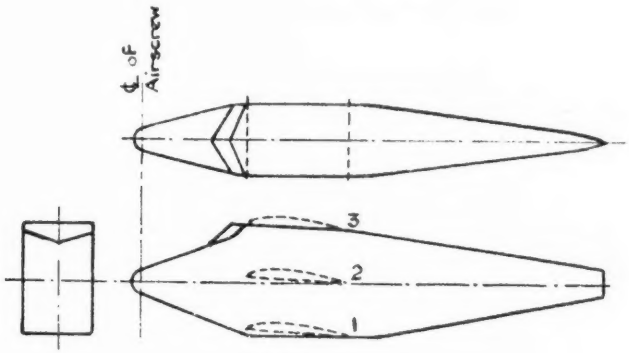


FIG. 16.

5.22. Turning now to the work of Muttray, we note that this was confined entirely to the low wing monoplane in which the under surface of the wing was roughly flush with the bottom of the body. The first series of tests was designed to explore the effect of variation of the axial distance of the wing from the nose of the body. Fig. 17 shows the experimental arrangements for these tests, the results of which may be summarised by the statement that the flow round the nose of the body appeared not to affect the wing characteristics until the leading edge was within one chord from the nose, but we must note that until the leading edge was less than one chord length from the nose it had not entered the region in which the parallel walls of the body commenced to curve off towards the nose in plan view. Further, the nose of the body had no taper at all in elevation. The conclusion cannot, therefore, be regarded as general.

5.23. In the next series of tests a comparison was made between a rectangular wing and a tapered wing of equal span, used in conjunction with the same body. The rectangular wing was the same as that shown in Fig. 17, while the tapered wing had a chord of 100 mm. at the tips and 300 mm. at the intersection of its straight leading and trailing edges with the longitudinal body axis. Up to a lift coefficient of 0.35 the interference of the body with the characteristics of the wings alone was about the same in the two cases, but at higher lifts the rectangular wing was spoiled more than the other by the presence of the body, the maximum lift of the combination being about 6 per cent. higher with the tapered wing.

* Models more nearly representing actual bodies—reference should be made to the Journal, *loc. cit.*

The author attributes the inferior behaviour of the rectangular wing to the fact that the body has a larger disturbing effect to the root of the wing than it does to the relatively larger root of the tapered wing.

5.24. Perhaps the most interesting of Muttray's tests are those dealing with the effect of changes in the cross-sectional shape of the body. For these experiments two bodies were made, streamline in plan and elevation (see Fig. 18), one having a square and the other an equilateral triangular cross-section.* Using these bodies in various symmetrical attitudes on a rectangular wing of 5 to 1 aspect ratio, Muttray was able to obtain four low-wing monoplane arrangements in which the sides of the bodies made angles of 45°, 60°, 90° and 120° with the

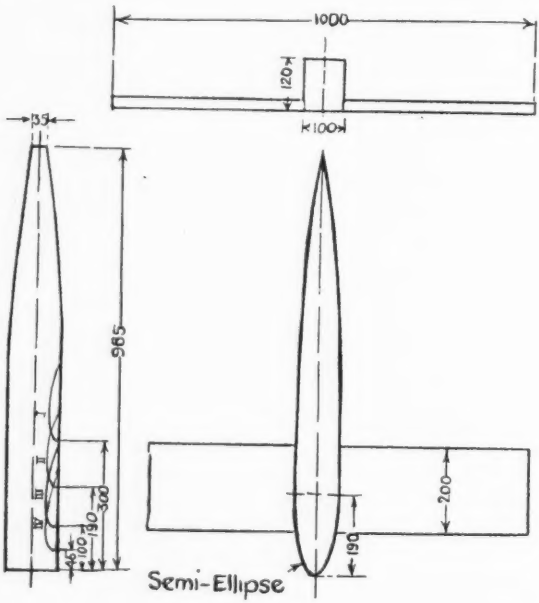


FIG. 17.

tangent plane to the lower wing surface. That shown in Fig. 18 is the 60° arrangement. The results for the 120° and 90° angles are indistinguishable from each other over the whole of the useful incidence range, except just near maximum lift, where 90° is slightly better as regards drag. When the sides of the body sloped at 60° the drag at all incidences was considerably augmented, by an amount increasing with incidence, and at 45° a further large drag increase was observed, particularly in the region of minimum drag. Further, the maximum lift at 60° and 45° was somewhat lower than for the other angles.

5.25. These results show definitely that the presence of acute angles of less than 90° between the sides of the body and the upper wing surface may be expected to lead to serious loss of performance. The reason for this would seem to be somewhat similar to that given for the increase of drag due to the divergence of the surfaces in the N.P.L. experiments. Although in Muttray's experiments

* It may be noted that tests on these two bodies without wings attached showed very small differences, so that the large effects found with wings added could not be attributed to differences in the bodies themselves.

the sides of the bodies were not tapering towards the tail at the positions of the wings, there was a quasi-expansion effect due to the divergence of the upper wing surface from the adjacent body surface when the sides of the body made angles of less than 90° with the wing, and this was intensified the more acute these angles became. Experiments with fillets produced improvements similar to those already noted, the best fillet being one of progressively increasing radius towards the trailing edge of the wing, and having a suitable fairing beyond it. With the best fillets, the results for the otherwise bad cases in which the body side sloped at 45° and 60°, were made practically as good as those for the good arrangements of 90° and 120°.

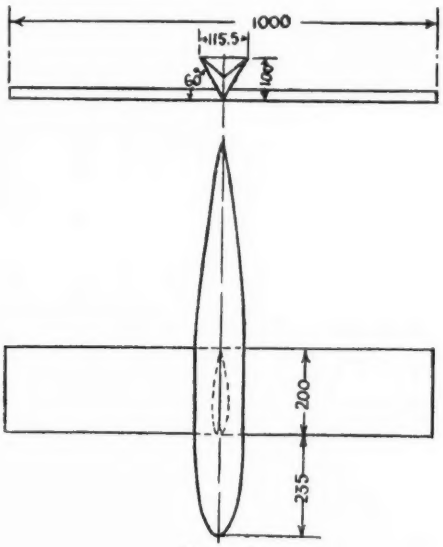


FIG. 18.

Summary of Conclusions regarding Body-wing Interference

5.26. We are now in a position to draw a number of general conclusions from the various experiments we have just considered. In general, body-wing interference is worse the better the aerodynamic shape of the body, being most pronounced when the body is circular in cross-section. It also appears to be greater for thick wings than for thin wing sections. The origin of large detrimental interference seems to be associated with divergence between wing and body surfaces, particularly when such divergence occurs on the upper surface of the wing. Sharp angles of less than 90° between wing and body surfaces also act unfavourably, especially when they occur in regions of divergence. On the other hand, when the surfaces converge in the direction of flow, sharp angles are less detrimental to performance. The evidence for the last statement is found in the N.P.L. experiments in which the interference drag at a given lift was progressively decreased as the wing, always with its upper surface in contact with the lower surface of the streamline body of circular cross-section, was moved upstream in relation to the body.

5.27. Of the possible arrangements in which the wings intersect the body, the worst in all cases is the low wing monoplane, while the best for bodies of

oval or circular cross-section is that in which the wings spring roughly from the maximum horizontal diameter of the body. For bodies with parallel vertical walls at the region where the wings are attached the difference in performance with variation of the vertical position of the wings is less marked, and there is evidence that a high wing position may be superior to the central position. It seems, however, that the shape of the body in elevation forward of the wing may have an appreciable effect, particularly when the wing is on the top of the body, and as the difference due to changing the vertical wing position on a parallel vertical-walled body is not great, the superiority of the high wing position in the case when it was most marked may well have been due to the particular shape of the nose of the body in that experiment.

5.28. As regards fairings or fillets, these have a large effect in improving otherwise bad combinations, but little or no effect in good combinations. Their efficacy lies partly in the reduction of divergence and partly in the suppression of sharp corners, particularly when used on the sensitive upper surface of the wing. With properly-shaped fairings the performance of a bad combination can be made little inferior to that of the best possible with the particular wing and body in use. The design of a fairing in any particular case is a matter for *ad hoc* tests, but the general guiding principles appear to be the increase in the radius of the fillet towards the trailing edge of the wing, so as to compensate for increasing divergence, and a suitably gentle fairing at the back of the fillet proper to avoid sudden changes of section.

5.29. Finally, there is the question of the effect of wing position on maximum lift. This has formed the subject of a recent report by Hartshorn,[8] who has examined the available model and full-scale results. From an analysis of the data obtained by Parkin and Klein, Hartshorn has divided the interference effect on maximum lift into three factors—aerofoil shape effect, body shape effect, and position effect—and has deduced approximate numerical values for each. As Hartshorn himself points out, these values must be used with caution, as they depend on data obtained at low Reynolds numbers where observations of maximum lift are known to be liable to differ appreciably from the full-scale values. It is, however, possible that the interference effect associated with any particular design is less sensitive to scale effect. An examination of the rather meagre information available from other model tests and a few full-scale measurements indicates rough agreement with the results of Hartshorn's analysis. He concludes that "the evidence is consistent in showing that the most beneficial combination (as regards maximum lift) is obtained with the wing flush with the upper surface of the body, and model evidence suggests that in this position the aerofoil characteristics are of secondary importance, whilst the most deleterious combination is that obtained with the wing flush with the undersurface of the body. In this position model evidence suggests that the aerofoil characteristics have a more marked influence on the interference effect, which is worst with a thick aerofoil. The effect of the body shape is seen from model evidence to be quite large, and there does not seem to be any simple characteristic of the body which has a dominating influence." The last statement is significant; body shape may well be more important than the other factors, and if it is, for some reason, essential to obtain the highest possible maximum lift with a combination of a given wing and body, *ad hoc* tests appear to be desirable.

§6. Nacelle-wing Interference

6.1. With the modern trend of design towards monoplanes with engine nacelles built into or supported by the wings, the question of nacelle-wing interference assumes importance. From an aerodynamic point of view it may be regarded as a somewhat extreme case of body-wing interference, in which the body has shrunk in relative size to a length about equal to or rather less than the wing-

chord, and the general principles regarding body-wing interference may be expected to apply.

6.2. A fair quantity of data has been amassed on this subject, the most important researches to note being those of Jacobs[9] in America, of Perring and Callen[10] at the Royal Aircraft Establishment, and of Ower and Hutton[11] at the N.P.L. All these agree in pointing to certain general conclusions. Thus, for nacelles supported clear of the wing from its upper or lower surfaces, serious interference drag is experienced if the nacelle is too close to the wing, particularly to the upper surface. The worst case of all is when a nacelle rests in contact with the upper wing surface with no attempt at fairing. Better, but still bad as regards interference, is the corresponding case with the nacelle below the wing.

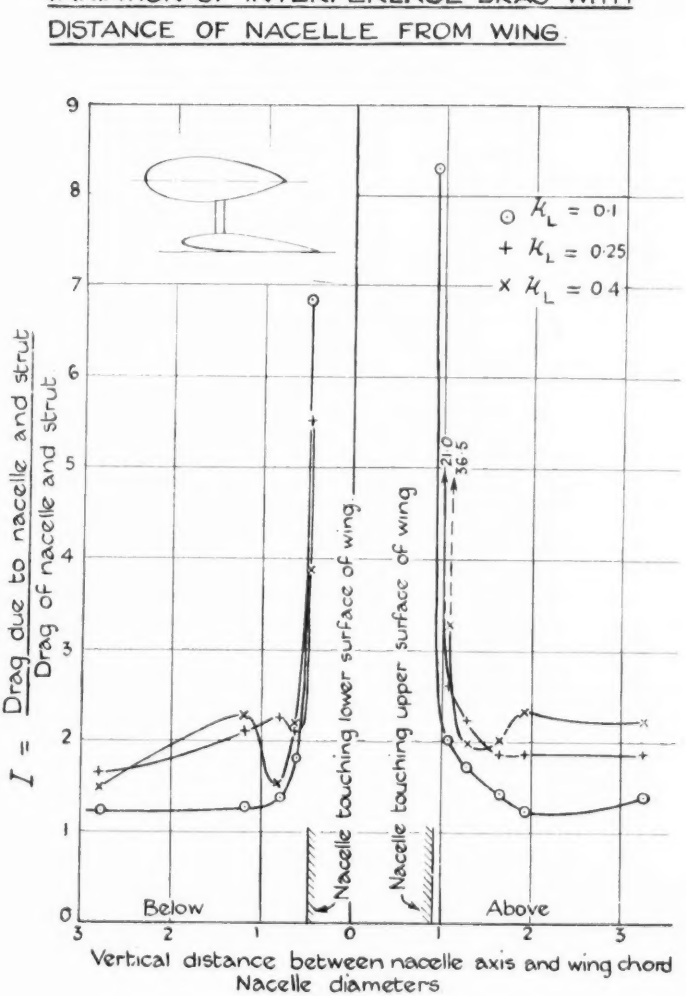


FIG. 19.

6.2. Quite a small gap between the nacelle and the wing is sufficient to reduce the interference considerably. This is illustrated by the curves of Fig. 19, in which the ratio of the increase in drag of a R.A.F. 31 model wing, due to the presence of a streamline nacelle and its supporting strut, to the free drag of the nacelle and strut is plotted, for three values of the lift coefficient, against the distance of the nacelle from the wing. Perring and Callen make the general recommendation that when a nacelle is installed above and clear of the wing its axis should be at least $1\frac{1}{2}$ nacelle diameters, but preferably 2 diameters, above the wing chord. The interference when the nacelle is below the wing is seen to be less serious, but even here the results obtained by Jacobs show that the nacelle axis should be at least 1 diameter below the chord. A few experiments on the high nacelle show also that the interference decreases somewhat as the nacelle moves forward on the wing. This is probably an instance of the phenomenon which leads to the similar result noticed above for a wing below a streamline body.

6.3. Although model results thus indicate the desirability of placing the nacelle some distance above or below the wing, it must be remembered that in any actual design the nacelle will have to be supported from the wing by a system of struts relatively much larger than the single small strut used in the model experiments at the N.P.L. and the R.A.E. Hence a stage will be reached when the drag of this supporting structure, which will increase with the distance of the nacelle from the wing, begins to exceed the decrease of drag due to putting the nacelle further from the wing. The position at which any further increase of this distance begins to be detrimental may occur within two diameters from the wing. This would appear to be a matter for test in any particular case, since it will depend largely on the form of supporting structure deemed necessary.

6.4. A feature of some importance noticed by Jacobs was that with a cowled nacelle below the wing the drag of 152lbs. (full-scale at 100 miles per hour), due to the addition of the nacelle and its supports to the wing, was reduced to 49lbs. by filling in the gap between the wing and the nacelle with a plasticine fairing,

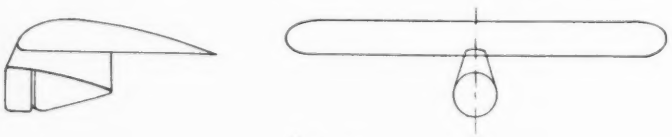


FIG. 20.

as shown in the sketch of Fig. 20. Jacobs points out, however, that there were indications when a similar fairing was tried in flight that the rotation of the slipstream caused a stall to take place on one side of the fairing. Although there is no direct evidence regarding the efficacy of a similar type of filling-in of the gap when the nacelle is above the wing, it seems clear that it would not produce so marked an improvement as it does on the underside, for, although the region of divergence between body and wing surfaces would be suppressed, the interruption of the lift grading along the span would be much more pronounced than when the nacelle is below the wing.

6.5. This consideration of the interruption of the normal lift grading may be taken as the predominant factor in those cases in which the nacelle is built into the wing. The less the irregularity introduced into the lift grading, the less will be the extra induced drag set up, and it would seem to follow that the more the nacelle protrudes above the upper surface of the wing the higher will be the interference drag. This deduction has been found to be in accordance with fact, for Perring and Callen showed that when the nacelle axis was about 0.6 nacelle diameters above the wing chord the interference drag was very high, but it decreased continuously as the axis approached the wing chord and was actually

negative when the axis and the chord coincided. It seems that the nacelle in such positions produces so slight a disturbance of the lift grading that the resulting induced drag is less than the reduction of drag of the nacelle itself due to its partial housing in the wing. Jacobs noticed similar instances of negative interference with nacelles whose axes were slightly below the wing chord when the surfaces were well faired at their junctions.

6.6. The principle of avoiding disturbance of the load grading receives further support from another experiment made by Perring and Callen. The arrangement of nacelle and wing shown in Fig. 20 was that in which the nacelle axis was about 0.6 diameters above the wing chord, and, as mentioned above, high interference drag was experienced. But when a slot was cut, as shown by the dotted line in Fig. 21, to allow air to flow over the upper surface of the wing immediately below the nacelle, the interference was considerably reduced.

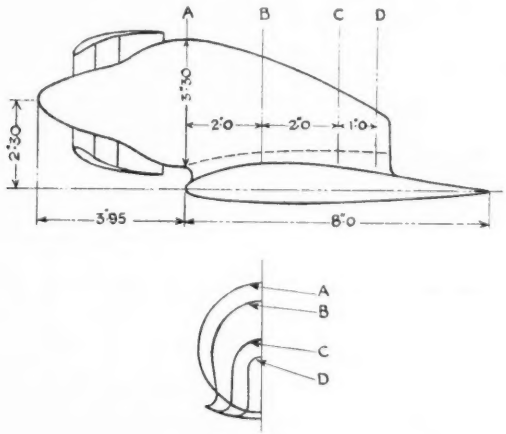


FIG. 21.

6.7. The same investigators found that a model fuselage placed in a representative position with respect to the wing had no important effect on the observed interference when the nacelle was above and clear of the wing, except at high incidences near the stall, when the interference which, without the body present, increased rapidly, fell off appreciably in the presence of the body. It is surmised that this was due to some shielding or screening action on the nacelle by the body. Confirmation of this view is afforded by the fact that the reduction of interference drag due to the body began at an earlier incidence when the nacelle was nearer the wing.

6.8. As regards the effect of the airscrew slipstream on the interference between wing and nacelle, Perring and Callen made a few tests in their investigation and found that in all except the case of worst interference, when the nacelle was built on to the wing with its axis 0.6 diameters above the chord, there was no large modification of the nacelle interference by the slipstream. For this one bad position the interference was considerably reduced by the slipstream, but the reduction was not enough to convert this otherwise bad combination into one that could reasonably be called good. This indication that the slipstream may have an appreciable " cleaning up " effect in cases of bad interference seems to be worth investigation, and I hope to have an opportunity of exploring it further.

6.9. We may consider now an aspect brought out by the N.P.L. experiments with a streamline nacelle and a R.A.F. 31 aerofoil. In addition to the overall interference measurements, observations were made of the pressure distribution on both surfaces of the aerofoil along chord lines situated at various distances from the nacelle axis. In this way it was possible to determine the load grading

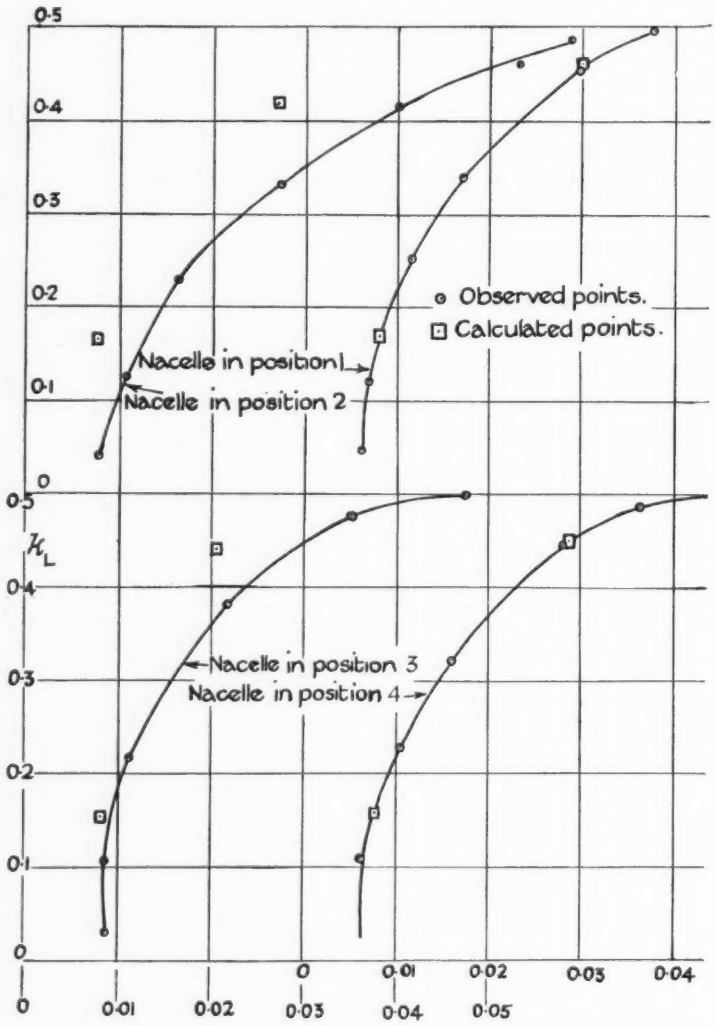


FIG. 22.

along the span both for the wing alone and for the wing in the presence of the nacelle in various positions. Now, if the effect of the nacelle on the wing is mainly to alter the distribution of lift along the span without causing any sections to stall, and if, in addition, the effect of the wing on the nacelle is small, the

drag of the combination should be substantially equal to the sum of the profile and induced drags of the wing and the free drag of the nacelle and its supporting strut. The induced drag was calculated from the measured lift grading by means of Fage's method[12] for a few positions of the nacelle with respect to the wing, and the drag of the combination, calculated by the process of summation indicated above, was compared with the overall drag measured at the lift corresponding to the integrated lift grading along the span.

6.10. Fig. 22 shows the results of the comparisons. For nacelle positions indicated as 1 and 4 the agreement is exact, whereas for positions 2 and 3 there is a large discrepancy, the calculations underestimating the drag at a given lift. In positions 1 and 4 the nacelle axis was respectively 1.67 diameters above the wing chord and 1.20 diameters below, whereas in positions 2 and 3 the nacelle was resting respectively on the upper and lower surfaces of the wing. The two latter positions, therefore, as we have already seen, were ones in which high interference associated with divergence were to be expected. This was found to be the case, for the pressures on the aerofoil near the nacelle in these two positions showed unmistakably that the flow had broken away from the surface. Thus the condition, enunciated above, that no stalling should be produced was violated and the calculations were not valid.

6.11. The conclusion to be drawn is that, provided the interference is not such as to produce an actual breakdown of flow over the wing or nacelle surfaces, the interference can be treated by aerofoil theory. A method of doing this when the pressure distribution is not known has been given by Wieselsberger in a lecture delivered to the International Engineers' Congress, at Tokyo, in October, 1929[13].

§7. Interference due to Wing Tanks, Cut-away Sections, etc.

7.1. When it is necessary to build into a wing protuberances such as wing tanks, or to cut away the centre section in order to improve the pilot's view, the general principle to be observed to minimise interference is the same as that which applies to built-in nacelles, namely, that the normal lift distribution along the span of the wing must be disturbed as little as possible. In connection with wing tanks no more need be said as, from an aerodynamic point of view, they do not differ from small nacelles. But regarding alterations to the chords of centre sections we may profitably refer to Muttray's work.[14] This shows clearly that, by adjusting the section and incidence of those parts of the wing where the chord is changed, we can compensate very largely for the change of lift and so avoid serious disturbances of the lift grading. Complete compensation is only possible at one attitude of the wing and this will naturally be chosen in accordance with the type of machine of which the wing is to form part. At other attitudes there will then be a slight, but usually unimportant, residual interference drag.

§8. Interference due to the Junction of Struts with Wings and Bodies

8.1. There is very little quantitative information regarding the interference effects of struts. It is difficult to obtain reliable data in the wind tunnel, partly because in scale models the struts are so small that the results obtained are subject to serious scale effect, and partly because the drag of struts is often critical to small angles of yaw and the effect of this, as a result of small errors in alignment, may well obscure the true interference effect. Figures given by Mr. J. D. North in his lecture to this Society in January, 1929,[15] show that the interference drag due to a single strut mounted on the upper surface of a wing (no details of the position on the chord or of the wing section are given), may be equal to the free drag of a considerable length of strut, and that the interference increases as the angle between the strut axis and the wing span is

decreased from 90° to 10°. There is some evidence also that the interference increases with the lift, but this is not conclusive. A few American tests[16] confirm the possibility of high percentage interference drag with struts on the upper wing surface (in this case they were inclined at 20° to the span) and show that the interference is considerably less for struts below the wing.

8.2. Both effects which appear to be established by these tests, viz., the increase of resistance as the angle between the strut and the wing span becomes more acute, and the greater sensitivity of the upper wing surface to interference are in accordance with the general principles that have been expounded in this paper. Similarly, the principles applicable to body-wing combinations may be expected to apply to the case of strut-body interference, in regard to which I have been able to find no published information. The difference between the junction of a strut or a wing with a body is only a difference of degree, not of kind.

8.3. A word is advisable regarding the interference effects of struts lying within the slipstream. Owing to the sensitiveness of the drag of many struts to wind direction, interference effects measured in the tunnel without airscrew running may be appreciably modified by the yaw produced by the rotation of the slipstream.

§9. Notes on Interference Experiments made in the Wind Tunnel

Measurement of the Drag of Streamline Bodies

9.1. In many cases the basic body used for a series of interference experiments in the wind tunnel will be one of streamline shape. Now it happens that the size of body that is usually tested in tunnels of normal size, together with the wind speed available, bring such tests within the range of Reynolds number between 10^5 and 10^6 approximately, where, as we have seen, the change from laminar to turbulent flow is likely to occur. Without large, high-speed or compressed-air tunnels it is not generally possible to conduct the tests at higher Reynolds numbers where turbulent flow in the boundary layer is well established, unless bodies too large for the tunnel are used. Moreover, it is found that, when a streamline body is tested within this critical range of Reynolds number, the position of the point along the surface at which the flow changes in character is not definite; it is sensitive to the amount of turbulence present in the air stream and so varies from one tunnel to another and frequently even from day to day in the same tunnel. Corresponding changes in the drag of the body will therefore occur, and these may well obscure drag differences due to different arrangements of the body and the rest of the structure which the experiments are intended to disclose.

9.2. For the true interpretation of such experiments, therefore, it is necessary either to know in every case the proportions of the body surface over which the boundary layer is respectively laminar and turbulent, or, alternatively, to cause the layer to be either entirely laminar or entirely turbulent. Now there is as yet no rapid experimental method of determining the transition point, and the laminar state of the boundary layer is stable only for very low Reynolds numbers. Hence the most promising practical expedient appears to be the production of a turbulent layer by artificial means. Recent researches at the N.P.L.[4, 17] have indicated two possible methods of inducing turbulent flow in the boundary layer, except for a small localised region near the nose of the body where it is doubtful whether any normal disturbance can cause the laminar flow to break down. However, this region can generally be so restricted in size that it represents an insignificant fraction of the total surface area.

9.3. The two methods in question are firstly the introduction of a cord network extending right across a transverse section of the tunnel a short distance upstream of the nose of the body; and secondly the attachment of a few

rings of fine thread or wire to the surface of the body in transverse planes near the nose. The former method can be dismissed with brief consideration, for it is not to be recommended for accurate work. In order to ensure that enough of the turbulence introduced may persist over the length of the body, it is generally found that the network has to be fairly close to the nose, or that rather large cords have to be used. In either event the tunnel flow is seriously disturbed, both as regards static pressure drop and velocity distribution. Further, it is not an easy matter to adjust the network in position.

9.4. As regards the effect of attaching rings of thread round the nose of the body, let us consider the case of a body whose boundary layer is mainly laminar. We fix one ring near the nose and thereby induce turbulence immediately behind it. Consequently the drag is increased, as more of the boundary layer is turbulent. But if conditions are such that the laminar type of flow is the more stable, the turbulence introduced will be damped out at some distance behind the thread, and another thread must be attached downstream of the first. If the only effect of the threads is to change the laminar type of flow in the boundary layer to the turbulent type, a stage will be reached when the drag attains a constant value which is unaffected by the addition of further threads. We may then expect the total drag of the body less its pressure drag to be equal to the surface friction of a flat plate of the same area (see para 3.5). Unfortunately, it is found that, in general, the effect of the thread is not confined merely to the production of a change in the boundary layer flow. Just as the discs changed the pressure distribution on the body,* so, usually, do the threads, if they are sufficiently large to be effective, and it is found that the total drag tends to increase continuously as threads are added. Each thread produces an increase in the form or pressure drag of the body. But, when this pressure drag is measured and subtracted from the total drag, the difference is found to be roughly constant at a given speed when sufficient threads have been fixed to the body, and the value of the difference is of the same order as the surface friction of a flat plate of the same area at the speed of the test.

9.5. We see, therefore, that the effect of the threads is to produce a body of somewhat different aerodynamic shape from that of the original body, but one whose frictional drag is nevertheless approximately that for turbulent flow. Moreover, provided enough threads have been added, its total drag will not depend upon the degree of turbulence present in the airstream. Hence, the body in this condition will be suitable for use in interference experiments. If the increased total drag is regarded as an objection, allowance can be made for it by determining the form drag of the body with and without the threads in place. This procedure is valid, for there is evidence that the pressure distribution on the surface of a streamline body is not affected by the type of flow in the boundary layer.

9.6. It only remains for us to consider how the experimenter is to know when he has attached enough threads to the streamline body he is using. As the drag at a given speed increases for every thread added, the criterion of constant drag when enough threads are in place, which would hold if the threads only induced turbulence, does not apply. The practical expedient is to attach the threads one by one and, at every stage, to make a few overall drag measurements at different speeds. Curves of the type shown in Fig. 23 will be obtained when the results, divided by the square of the speed, are plotted against speed. For the curve labelled "bare body" in Fig. 23 the shape indicates that the boundary layer was mainly laminar at all speeds. For the 1-thread and 2-thread curves it was laminar at low speeds and became more and more turbulent with increase of speed, while with 4 or more threads the curves indicate that the layer was turbulent at all speeds over 50 feet per second. The last three curves,

* Fig. 4.

embodying a rising portion at low speeds followed by an appreciable speed range over which the drag coefficient falls with increasing speed, show the type of curve to be sought. If the speed at which it is decided to conduct the interference tests is in the Reynolds number range over which the drag-coefficient curve of the body is falling, as in Fig. 23, it may confidently be accepted that the boundary layer is in the turbulent state. Thus, if in the case represented by Fig. 23 the wind speed chosen for the tests were 50 feet per second, four threads would be enough.

9.7. As regards the size of the threads, in the N.P.L. experiments[17] to which Fig. 23 relates the body was 8in. in maximum diameter and 2 feet long,

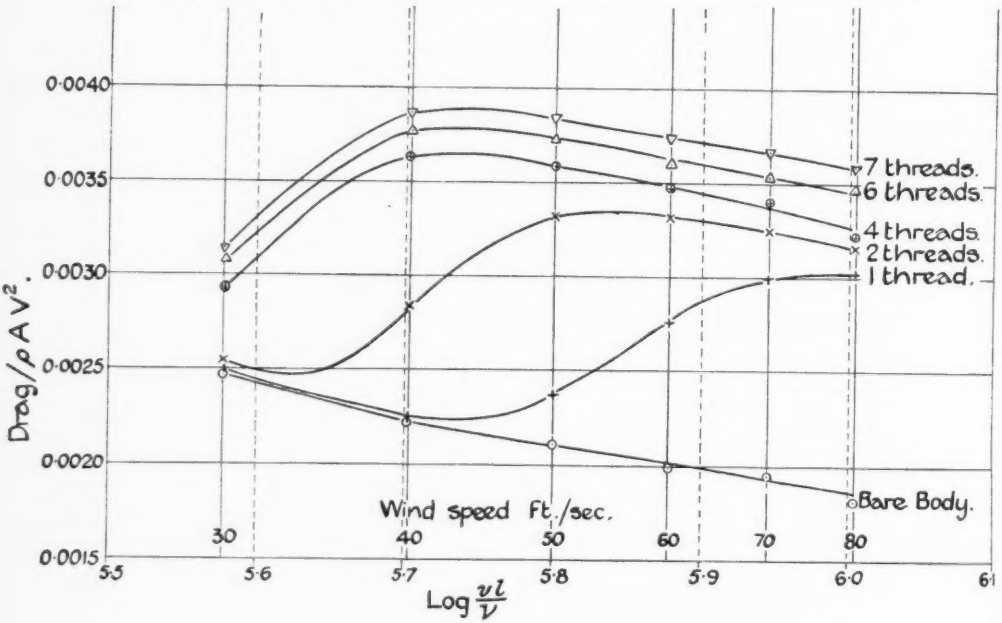


FIG. 23.

while the threads consisted of fishing gut 0.014in. thick, stuck to the body surface with very small quantities of seccotine. The first thread was in the transverse plane 1 inch from the nose of the body, and the distance between neighbouring threads was 1 inch.

The Use of Large Models in Small Tunnels

9.8. In order to determine in the wind tunnel the interference of, say, a nacelle on a wing it is useless to attempt to work with a model of the nacelle and engine of such small size that detail cannot be faithfully represented, or that scale effect will invalidate the application of the model data to the full scale. The scale of the smallest complete model that can be used is thus fixed by the minimum size of the nacelle model dictated by such considerations. If we assume for this minimum size the somewhat low value of 6in. maximum diameter, the length of the wing chord, estimated from the average proportions of modern aeroplanes, will be about 20 inches. Thus if a complete model is to be tested its wing span may well be too great to allow the work to be done in any but a large wind tunnel.

9.9. It has therefore become the practice to work with partial models in which the nacelle is made to a convenient scale and only part of the wing span is included. But in many cases it has been found that this procedure does not give results that agree with those subsequently measured in flight or with complete models, despite the fact that one or other of the usual methods has been adopted to eliminate wing tip effects on the partial model and to attain "infinite aspect ratio" characteristics. Among the various causes that may contribute to the discrepancy, there is the possibility that the region over which the flow past the wing is changed by the presence of the nacelle extends along a greater length of span than can be included within the limits of the tunnel walls. It is therefore desirable to know how far the interference effect of the nacelle may extend out over the span, and some tests were undertaken at the N.P.L.[11] to furnish information on this point. The method adopted was to determine the pressure distribution over a number of sections of an aerofoil parallel to and at various distances from the median section, and to see how far it was necessary to proceed out along the span to bring the measurements made on the wing alone and on the wing with a nacelle at the median section into sensible agreement.

9.10. The results of these tests indicated that, although the main interference occurred, as expected, in the vicinity of the nacelle, the influence of the nacelle extended in some cases for 6 or 7 diameters out along the span. If, therefore, direct agreement over the whole flying range is to be expected between measurements made in the tunnel and in flight at least this length of span must be included in the model. This statement must not, however, be taken to mean that no reliable design information can be obtained from experiments with large-scale partial models in a tunnel having a working section of as little as 4ft. side. There are, in fact, good grounds for the belief that tests of this kind may be made to provide valuable data. In the first place, a somewhat shorter span can be used if small errors are not objectionable, for the magnitude of the interference falls away outwards along the wing. Secondly, since the interference is found to increase with lift, it is probable that, from tests with partial models in small tunnels, results may often be obtained which are directly applicable to design without serious error over the lower range of lift coefficient—possibly as far as moderate climbing attitudes.

9.11. The interpretation of such model tests has formed the subject of an investigation at the R.A.E.[18] It is obvious that, since the interference will in general affect the lift as well as the drag at a given attitude of the model, wing incidence is not a suitable basis on which to compare the effect of interference on performance. What is required is a knowledge of the change of drag at a given lift. The question then arises as to the value of the lift coefficient of the partial model, in which the small portion of the wing span retained is considerably affected by the nacelle, that corresponds with a given lift coefficient of the complete machine where most of the wing is affected only slightly, or not at all. The R.A.E. work shows that corresponding conditions exist when the local lift coefficient of the wing adjacent to the nacelle is the same in the two cases.

9.12. A further material consideration now arises, to wit, the effect of the constraint of the tunnel walls on the lift coefficient of the model wing. This is bound to be large in view of the large dimensions of the wing with respect to the tunnel, and in order to remove doubt as to the value of the lift coefficient at the centre of the model wing, the R.A.E. recommend that the latter should be given infinite aspect ratio characteristics by carrying the span fairly close up to the walls and continuing it right across the tunnel with lengths of aerofoil carried from the walls and separated from the central aerofoil by very small gaps. Corrections due to tunnel wall interference on the wing characteristics are then negligible, and the attitude of the effectively infinite wing in the tunnel at which its lift coefficient is the same as that at the centre of the full scale wing can be calculated by ordinary aerofoil theory.

9.13. In this way agreement can be obtained between full scale and properly interpreted model results when sufficient span is included in the model to embrace the bulk of the interference, but if the span is too short, the agreement is impaired at the higher lift coefficients. Provided, however, the wing-nacelle combination is not so bad as to promote a local breakdown of flow equivalent to the stalling of part of the wing, there is another possible method that can be employed in such cases to extend the range of validity of the model tests. For we have seen (para. 6.11) that, subject to the condition of no breakdown, the problem can be treated by aerofoil theory, so that tests of partial models may be used as a basis for the calculation of equivalent full scale results. One method of carrying out such calculations has been indicated by Wieselsberger in the lecture to which earlier reference has been made[13]. In brief, it treats a built-in nacelle as part of the wing having different geometrical incidence from that of the remainder, and, in the simplest case, it involves a knowledge only of the aerodynamic properties of the main wing itself. Unfortunately the method involves a great deal of laborious arithmetic, but apart from this there is no particular difficulty in applying it, and the accuracy appears to be very good, at all events for the examples given.

9.14. When the wing-nacelle combination is such that part of the wing does stall, aerofoil theory cannot be used, but even here tests with partial models will yield valuable data, despite the fact that the span may be too short to include all the interference. For, in such cases, the greater part of the interference drag will be due to the breakdown of flow and its source will therefore be in the local region near the nacelle which will be included even in a model of relatively short span. A convenient and simple method of identifying such centres of bad interference due to local stalling involves the use of light streamers or wool-tufts; this is too well known to need elaboration, but it is doubtful whether it is employed as often as its extreme usefulness would seem to warrant.

§10. Conclusions

10.1. The conclusions to be drawn from the mass of data discussed in this paper have been already given in detail in the separate sections. I have thought such a course preferable in a paper of this length as the conclusions do not then occur so widely detached from the evidence on which they are based as they do if a statement of them is reserved for the final section. I shall now, therefore, merely recapitulate briefly the principal deductions to which the evidence points.

10.2. Firstly, as regards the characteristics of streamline bodies. The most important conclusion here is that changes of the pressure distribution on the surface, due to the presence of excrescences, play an important part in the interference effects, and the possibility of turning such changes to advantage by suitable arrangements of the inter-acting surfaces must be remembered. It seems that one result of this type of interference is that we can get negative interference drag by attaching an excrescence to the surface of a streamline body at points inside a short region near the nose.

10.3. With combinations of wings and bodies or wings and nacelles, it is important to avoid as far as possible disturbance of the normal lift distribution along the span. Any such disturbance leads to an increase of induced drag. Excrescences on the upper surface of a wing cause greater disturbance of the lift distribution than corresponding excrescences below. Regions of divergent flow are detrimental to performance, particularly when one of the surfaces is the upper surface of a wing. Sharp corners also are bad when they occur in divergent regions, but are apparently not seriously unfavourable when the flow is converging.

10.4. The practical results of these considerations on body-wing combinations are briefly as follows: With bodies of circular or oval cross section the best position of the wings, when they intersect the body, is that in which they spring approximately from the horizontal centre line. The worst is the low wing monoplane

with the upper wing surface touching the bottom of the body. Somewhat better, but still very bad, is the corresponding combination with the wing on the top of the body. The characteristics of both these bad combinations, however, and, in fact, of many others, can be greatly improved by suitable fairings at the junctions of wing and body surfaces, which suppress sharp angles and reduce divergence. When the body has vertical sides at the position of the wing roots the difference between high and low wing positions is much less marked, although, even in such cases, the low wing appears to be at a slight disadvantage.

10.5. The principles applicable to body-wing interference govern also the design of wings carrying nacelles or wing tanks. Thus, sharp corners and divergence of the surfaces should be avoided, and as uniform a load grading as possible should be preserved along the span of the wing. When a nacelle is clear of the wing it should be placed with its axis at least $1\frac{1}{2}$ diameters above the wing chord or 1 diameter below it. The axes of built-in nacelles, on the other hand, should be as near as possible to the chord. For wings with cut-away centre sections the dominant consideration is the avoidance of disturbance of the normal lift grading; where the chord is reduced, incidence or camber, or both, must be compensatingly increased.

10.6. Cases of interference which have been neglected altogether, or only briefly discussed for lack of evidence (such as struts and wings, or struts and bodies, for example) would appear to be subject to the two important considerations of absence of divergence and absence of sharp angles which may be regarded as established from the work discussed in this paper.

References

(1) The Streamline Aeroplane. Journ. Roy. Aero. Soc., XXXIII., 1929, p. 357. B. M. Jones. See also The Importance of Streamlining in Relation to Performance. R. & M.* 1115. B. M. Jones; and Skin Friction and the Drag of Streamline Bodies. R. & M. 1199. B. M. Jones.

(2) Collected Reports on British High Speed Aircraft for the Schneider Trophy Contest of 1927. R. & M. 1300. Compiled by W. L. Cowley.

(3) Experiments relating to the Flow in the Boundary Layer of an Airship Model. R. & M. 1268. Simmons.

(4) Investigation of the Boundary Layers and the Drags of Two Streamline Bodies. R. & M. 1271. Ower and Hutton.

(5) Experiments on the Drag of Two Streamline Bodies. Lock and Johansen. In preparation for publication in the R. & M. Series.

(6) The Interference between the Body and Wings of Aircraft. Aeronautical Research Paper, University of Toronto, No. 22. Parkin and Klein. See also Journ. Roy. Aero. Soc., XXXIV., 1930, p. 1.

(7) Investigation of the Effect of the Body on the Wing of a Low-wing Monoplane. Luftfahrtforschung, II., 1928, p. 34. H. Muttray.

(8) The Influence of a Fuselage on the Lift of a Monoplane. R. & M. 1344. Hartshorn. See also Aircraft Engineering, III., 1931, p. 201.

(9) The Drag and Interference of a Nacelle in the Presence of a Wing. N.A.C.A.† Technical Note No. 320. Jacobs.

(10) The Drag and Interference of a Nacelle when installed on the upper surface of a wing. R. & M. 1414. Perring and Callen.

(11) The Interference of a Streamline Nacelle on a Monoplane Wing. R. & M. 1395. Ower and Hutton.

(12) On the Theory of Tapered Aerofoils. R. & M. 806. Fage.

(13) The effect of Built-in Engine Nacelles on the Air Forces of a Wing. Wieselsberger.

(14) Recent Measurements on Aeroplane Wings with Cut Away Portions. Zeitschrift für Flugtechnik und Motorluftschiffahrt, XX., 1929, p. 161.

(15) The Technical Development of the Aeroplane. Journ. Roy. Aero. Soc., XXXIII., 1929, p. 1. North.

(16) Interference Effects and Drag of Struts on a Monoplane Wing. N.A.C.A. Technical Note No. 365. Ward.

(17) The Measurement of the Drag of Small Streamline Bodies. R. & M. 1409. Ower and Hutton.

(18) The Validity of Drag Tests on a Large Scale Model in a Small Closed Wind Tunnel. R. & M. 1371. Bradfield and Perring.

* R. & M. = Reports and Memoranda of the Aeronautical Research Committee (British).
† N.A.C.A. = National Advisory Committee for Aeronautics (U.S.A.).

Discussion

The President: The paper had led him to wonder whether the streamlined shapes of racing motor cars were right, since they travelled so near to the ground. If it were right in the case of a dirigible, for instance, that from the point of view of interference, it should be at least one length from the ground, then the motor car could not be right.

Mr. J. D. North (*Fellow*): He would be the very last to make any sort of criticism, as Mr. Ower had suggested might be done, because the type of models which had been used for the experiments described were not representative of aeroplane design. If they had been, the whole object of the series of experiments which Mr. Ower had carried out would have been missed, because the object of the experiments was not so much to add to their general statistical knowledge of isolated interference effects which occurred, but rather to find some rational ground to understand the reason for them. The conception, put forward by Mr. Ower, of the expanding air stream was an extraordinarily valuable means of visualising something which others who had carried out this sort of work had felt but had not been able to grasp. That one conception alone would entirely justify all the work that Mr. Ower had done on this subject, and no doubt it would enable them to understand very much better what they were doing in their *ad hoc* experiments. One found that as the result of experience one was able to eliminate these interference effects, but it had been somewhat difficult to understand exactly what one was doing. The conception of expanding flow would make the alterations, which gave advantageous results, much more plain as between one set of combinations and another.

Referring to Fig. 4, dealing with the effect of discs on pressure distribution on a streamline body, Mr. North said he presumed that those pressures represented the sums of the pressures around the circumference at each point. Of course, the local influence would appear very much more severe, but, notwithstanding the fact that the points in the curves represented the sum of the interference, it seemed remarkable how very quickly the ordinary pressure distribution was restored. The cross-section of this arrangement was like a threepenny piece on a five-shilling piece, and there was only point contact between the plate and the body. Possibly the fact that there was only point contact might be one of the reasons why the pressure was restored so quickly. Of course the annulus experiments went to the other extreme in that respect; but there were so many other variables entering into the annulus question that it was difficult to say whether, if the excrescences had been approximately square plates instead of circular, the effects would not have been carried further along the body. If they were localised there seemed to be reason for local modification of the shape of the body in order to get away from the adverse interference effect, even if one had to put the obstructions in unfavourable places. Although one knew that the proper place for a windscreen was near the nose, one could not put it there if the pilot happened to be seated halfway down towards the tail. What did seem important was to have a local curve of the body so that the reduced pressure might give an up-wind component; they might be able to induce that effect by local modification of the body shape, although it did not occur actually in the nose of the body.

Discussing the problem of interference between the wing and body, and Mr. Ower's series of experiments on wing roots, Mr. North said that unfortunately there were very many ways in which one could express the results of interference experiments. He wondered whether Mr. Ower had considered what would be the effect if the whole of the wings experimented with had been made of the same actual span instead of having the same amount of root projecting from the body, and the total drag of the combination plotted on the lift basis. That was the method which was customary; it gave a picture which was rather

easily understood, and one had to think of the conditions of drag and of lift simultaneously. Mr. Ower had adopted his method, of course, for another reason.

There was some reason to believe that what was important in this combination was not so much the position of the wing as the angle at which the wing struck the body. In other words, if a wing were in a low or a high position and if one bent the root of the wing round so that it struck the body normally instead of obliquely and so that one avoided the high expansion rate condition, one could get results just as good with that high or low wing position as with the middle position, although it was sometimes a matter of structural difficulty to carry that out effectively.

Mr. North was pleased to note, from the reference made to Muttray's work, that the ideas he had put forward with regard to maintaining the pressure distribution across the cut-away had been borne out by subsequent experiments.

With regard to struts, Mr. Ower had properly pointed out that no experiments on the interference of struts were of any use unless one started with struts large enough to have a reasonable resistance coefficient. What one was concerned with was the testing of the relationship of the flow of air round the strut and round the aerofoil, and not the geometrical effects of the actual members one on another. In his experiments to which Mr. Ower had referred the struts used were fairly large, at least $\frac{3}{4}$in. to 1in. long, and they were inside the range of reasonable coefficients. Certain experiments had shown that if one used struts about the size of a quarter streamline wire one could get interference effects about three times the total drag of the struts full-scale.

An interesting result had been obtained when testing the interference of crossed axles. Quite a large interference which was found to occur even on a model of fair size had disappeared entirely when the model was enlarged to full size. In that case the intersectors were locally cylindrical, and, of course, the conditions for proper cylindrical flow were pushed very much higher up the scale.

Generally speaking, in regard to the application of these results one should remember that the road to damnation was marked very much more surely than was the road to salvation, and he thought that if designers did not follow Mr. Ower's general suggestions they would almost certainly go wrong; but he had found that there were other factors entering into it, which made it necessary to carry out *ad hoc* tests before one could be quite sure that one was on the right road.

On airscrew interference he and his colleagues had done quite a lot of work and had obtained some very surprising results; they had found that the interference effects with three airscrews were quite different from those obtaining with one airscrew in regions aft the tailplane. They were a long way still from understanding what the effects of airscrews might be. One important factor was that airscrews themselves changed the pressure distribution along the span of a wing and increased the engine drag. When there was a large amount of wing in the slipstream, that matter could become very serious. Actually, there was only about 20 per cent. of the increase of lift that one might expect to get if one merely assumed that the lift was increased in proportion to the velocity of the slipstream.

Mr. North was rather surprised that Mr. Ower had not mentioned the interference effect in the neighbourhood of the empennage, because that was of great importance. The interference effects of tailplanes were fairly serious; they had been known for a very long time, but had been covered up by such terms as "tailplane efficiency." They were very real, and some exploration in that region would constitute a very useful addition to the work already done.

Finally, he expressed appreciation of the value of Mr. Ower's work. A considerable amount of reading of the paper and comparisons with similar

isolated results would be necessary in order that one could derive full benefit from it.

Mr. RELF (*Fellow*): He did not wish to take up much time as he thought the greatest interest would come from the criticisms of those outside the N.P.L. There were, however, two points he would like to mention, one of a rather fantastic nature and the other eminently practical. Mr. Ower had pointed out the great reduction of drag that would be achieved if they could make the boundary layer flow remain laminar at high Reynolds numbers. This may not be so impracticable as appears at first sight, indeed there are those who are inclined to believe that nature has already done it. From the observed speeds at which porpoises swim and the horse-power which the physiologists will allow them to suppose they can develop, it appears impossible to account for the speed unless their drag coefficient is that appropriate to a laminar boundary layer. If this be so, it is still very doubtful whether a similar performance can be imitated by " dead " matter. The problem is analogous to that of the bird, which when carefully set up in the attitude of flight and tested in the wind tunnel only yields a lift/drag ratio of 4 or 5, whereas the living bird can certainly glide at finer angles than this would indicate.

The point of practical importance was to draw attention to the connection between the interesting lecture of to-night and the fascinating demonstrations of fluid flow recently given before the Society by Mr. Farren. They do not yet make as much use as they might of the visualisation of flow. The regions of " divergence " which Mr. Ower has mentioned can be seen very clearly by the use of smoke or streamers and it should be possible to find the correct form of fairing to remove the turbulence by the use of visual methods. It would be very instructive to take one of Mr. Ower's bad cases, and to fair the divergent region until smoke or streamers showed steady flow parallel to the surface at all points. The drag of the resulting form would then be measured to see how nearly the pure skin-friction drag had been realised. One of the difficulties of this kind of experiment was to devise a suitable method of visualisation which can be applied to all parts of the model, and this question was being considered at the laboratory.

In conclusion, Mr. Relf said he should like to add his congratulations to those of other speakers. He thought that Mr. Ower had treated a difficult subject in an extremely lucid and interesting manner.

Mr. HOLLIS WILLIAMS (*Associate Fellow*): One gathered the impression, from Mr. Ower's tests on monoplane wings, that the low-wing monoplane was an absolutely hopeless proposition. That was unfortunate, because there were many tactical and operational advantages in using a low-wing monoplane.

This impression would perhaps have been avoided if the statement had been made by Mr. Ower that provided divergent flow was satisfactorily guarded against, then to avoid lift interference leading to vorticity and increase of induced drag it was necessary to maintain the load distribution over the wing, both in side and front elevation.

Provided this condition is satisfied it is possible for the low-wing monoplane to be equally efficient with the high-wing monoplane, and there are wind channel tests on record of a machine assembled as high and low-wing monoplane which bring out this point (Fairey Postal monoplane).

Mr. H. C. H. TOWNEND (*Associate Fellow*): Mr. Ower stated that while the drag coefficient of an annulus was about 1.7 times that of a circular disc, the interference effect of the annuli was considerably less than that of the discs, and Mr. Ower had attributed this partly to the fact that some portions of the annuli were situated in the retarded boundary layer, where the flow was not so rapid as it was outside. There was possibly another way of looking at the matter, as had been hinted by Mr. North. It was that if one placed a streamline body inside an annulus one practically removed half the edge of the annulus

straight away. It seemed to him that most of the drag of the discs and annuli resulted from the discontinuity produced at the edges, and that if one removed nearly 50 per cent. of the edge one might expect to reduce the drag by a corresponding amount. Mr. North's remark concerning the point contact made by the circular disc with the surface of the body was relevant here, because in that case one only distorted the flow round the disc over a relatively small part of its circumference.

Mr. W. O. Manning (*Fellow*): Though the paper had been of very considerable interest to him, he looked forward with even greater interest to the results of Mr. Ower's further experiments on models with propellers. It had been stated that the interference in the case of the Schneider Trophy monoplane was extraordinarily small; yet Mr. Ower had described it as having its wing in very nearly the worst position. Mr. Manning suggested that very likely the propellers had a very considerable influence in that particular case. He had been told that some experiments carried out in Germany had shown that the phenomenon of buffeting was very much modified in the case of the low-wing monoplane when the propeller was running. Another point of some interest in this connection was that the German soaring machines, which had been very carefully developed to give optimum lift drift ratios, gave the best results when the wing was rather high over the body—probably at a height of a quarter or half the chord. According to Mr. Ower's figures, however, that was not quite the best position.

Mr. Manning suggested, in connection with a remark made by another speaker, that if it were possible for a fish to secrete a fluid which was of considerably greater viscosity than water, and still very soluble in water, and that if that fluid oozed out through the skin as the fish passed through the water, there might be a viscosity gradient reaching out a short distance from its body. If such a gradient existed it might destroy entirely the turbulent type of flow.

Mr. Simmons: As a colleague of Mr. Ower, he appreciated the difficulties to be contended with in investigating this most unattractive subject, and Mr. Ower deserved to be congratulated upon having made it so interesting. Mr. Simmons added that he was familiar with the method of testing models on spindles, and big interference resulted from attaching very small spindles to streamline models. In one case a pin was placed on a streamline model and had considerably increased the drag. Later experiments had shown that the effect of such obstruction was to distort the flow in the boundary layer. There was a forward flow set up in the boundary layer, due to the presence of the obstruction, and it had enlarged the area of turbulence considerably. In fact, with an ordinary cylinder placed normally on a flat plate in the wind tunnel the flow stretched round the cylinder for a distance at least equal to the radius of the cylinder, and in that distance eddies were set up, which made the flow extremely turbulent. That was one method of obtaining an interference effect due to the direct presence of an obstruction, merely by causing premature turbulence. Until three days ago he would probably have agreed with Mr. Ower that the flow in the boundary layer was in a very uncertain condition, and might vary from day to day; but, as the result of a few experiments on an airship model, he was disposed to alter his opinion. The airship model was about 80 inches long, and he was locating the position of breakdown by means of smoke. It was perfectly definite that in front and over the major part of the body the smoke adhered to the surface as a very thin sheet, but when breakdown occurred it became tenuous, and the position was very well marked. The first experiments were carried out three weeks ago. Three days ago, however, he had had occasion to try to repeat his observations, but had found that he could not do that. Then he had noted that the model was covered with dust. On wiping away the dust to within three inches of the nose of the model, however, nothing had happened. Then he had cleaned the nose, and had found that the transition point, which had changed from a point

20 inches from the tail to a point about 40 inches from the tail—*i.e.*, it had moved through a distance of 20 inches at this particular wind speed—had gone back to the point 20 inches from the tail. That was the effect of the dust which had accumulated on the nose of the model in three weeks. On looking up a paper on gliding, by Herr Lippisch, read before the Society, he had found that reference was made therein to that very thing; therefore, it was not by any means a new discovery on his own part. He mentioned it merely as another cause of interference, and rather an unusual cause, which would perhaps explain some of the discrepancies found in the past when dealing with boundary layer flow.

Dr. A. P. Thurston (*Fellow*): He imagined that the author's results relative to the interference of a body and wings agreed exactly with what he had always thought. But his reasoning was as follows: If there were two planes, one above another, as in the biplane, they knew very well that owing to interference the bottom plane lifted only about 80 per cent. of the top plane. The body might be considered as a kind of aeroplane forming with the wing a biplane. As the wing was approached by the body there was interference, which became worse and worse until the two touched. In the case of the wing being touched underneath—since the lift on the bottom of a wing was only about half that on the top—the interference was not so great as it was if the wing were placed at the bottom of the body. When the wing was exactly in the centre of the body the combination had ceased to be a biplane and consequently the interference of the biplane was cut out and the combination was to be regarded as a monoplane. He had argued similarly on many points in aeronautics and had found that the argument was effective, and that by paying attention to this matter one could reduce interference and drag. The same argument applied to the case of wings and nacelles.

Another point of great importance was the interference of one body upon another in the same run of air. Before the war he had published papers showing the effect of placing one body in front of another, using every type of shape—squares, circles, ellipses, and so on—and in every case he had found that there was a position at which the drag could be reduced to a minimum, actually below the resistance of one body alone. With a main plane and a rider plane the same thing occurred when there was a gap, or, as Mr. Handley Page called it, a slot.

A point mentioned in the paper, and which he suggested should be further investigated, in view of its great importance, was the effect of interference on any body if another body moved in front of it or over it. To explain his meaning, he said that if one pulled out the plug of a bath containing water, a whirlpool was set up above the entrance to the waste pipe and air was sucked in through the hole. If one moved a plane across the whirlpool, the latter immediately collapsed, and there was a different type of flow. Again, if one put a shot through the centre of a waterspout, the transition of the moving body destroyed the eddying effect of the air and the water, and the waterspout collapsed. Precisely the same thing occurred in connection with air eddies set up by aerodynamic bodies, and if one introduced a little moving plane or body in the right position it would cut out some of the eddying. Another point of importance was that interference effects might be utilised to reduce the resistance of a combination of aerodynamic bodies—and that interference effects were of maximum use in securing stability and control. The use of rider planes in front of main planes, he maintained, should be for the purpose of securing stability and control by reducing the resistance when one wanted to reduce it, increasing it when one wanted to increase it, and by introducing another type of flow, *i.e.*, ensuring a bigger lift when one wanted that bigger lift and vice versa. He ventured to maintain that something of this kind could be ensured by utilising moving planes in association with fixed planes and bodies.

Mr. J. L. NAYLER (*Fellow*): As divergent flow results in an increase of drag on models, any new methods that can be developed to show up such divergence in a wind tunnel may prove of value, as pieces of silk, tufts of wool and titanium tetrachloride are all coarse methods. It may be worth trying air nearly saturated with water vapour. This would be possible in a completely enclosed return flow type of tunnel and he believed that quite a small expansion as would result from diverging flow would tend to precipitate the water vapour, and the resulting cloudy patch would show where undue interference was being created.

Mr. W. G. A. PERRING (*Associate Fellow*): Mr. Ower had given them in his paper a very useful summary of the experiments carried out to investigate the problem of aeroplane interference and he would like to refer briefly to some of the features of this work discussed by the paper.

The author draws attention throughout the paper to the serious interference that often occurs when the air passing a surface has to expand in diverging streams. The speaker met an interesting example of this type of interference during some experiments upon a helmet type of cowling for a radial air-cooled engine. With this type of cowling a somewhat constricted valley is formed between the helmets on neighbouring cylinders and the air streams passing through these valleys contract until they reach the plane of the engine centre, the streams then expand as the cowls blend into the fuselage and the fuselage diameter increases. It was found during the experiments that the reduction in drag due to the cowling was very susceptible to the width of valley in the plane of the engine centre, which of course determined the degree of divergence taking place over the rear of the cowling.

Another point discussed by the paper is the subject of wing and body interference, and the effect of " fillets " on this type of interference is considered. The effect of these " fillets " on the wing and body interference has as yet only been studied without slipstream. It would be interesting to know to what extent the results given in this paper are modified by the addition of an airscrew slipstream. In a biplane he once had occasion to test, it appeared from the evidence of an exploration with a fine silk thread, that the airscrew slipstream " cleaned up " the otherwise very bad flow conditions that existed at the junction of the lower plane with the fuselage, thus rendering the fitting of a fillet unnecessary. In this aeroplane the fuselage was approximately elliptical in cross section and the lower plane faired into the underside of the fuselage forming with it an acute angle of about 60° or 70°. It therefore approximated very closely to wing position L tested by the author.

In the paper it is suggested that the good results obtained for the high wing position in the case of Messrs. Parkin and Klein's tests were probably due to the nose shape of the body used for these tests. He suggested that it is equally likely that the transverse shape of the fuselage used for these tests may have been responsible for the results obtained. In this same connection, too, it is equally possible that the transverse shape of the bodies used by the author for the wing and body interference tests had an important influence on the results regarding wing position.

REPLY TO DISCUSSION

Replying to the President's suggestion that the streamline shapes of racing motor cars might be improved upon, he supposed that what one should aim at in this connection was to prevent the boundary layer from breaking away anywhere on the surface and to evolve by tests the shape which had the lowest form drag. Of course, in such tests a flat plate representing the ground should be placed in the appropriate position beneath the model. Although this did not reproduce exactly the effect of the ground it was probably a good enough approximation.

Having expected to be hauled over the coals in regard to the out-of-the-way shapes he had used for his experiments, he was relieved and gratified that so far from criticising them, Mr. North had expressed approval of the scheme adopted. With regard to Fig. 4, Mr. North was correct in supposing that the pressures shown there were the mean pressures round a circumference of the body. Mr. North had expressed surprise that the normal pressure was restored so quickly behind the discs and had suggested that in the case of square plates the disturbance might have extended further along the surface. As a matter of fact he (Mr. Ower) had done some force measurements, similar to those with the discs described in the paper, with rectangular plates, but had not thought the results sufficiently interesting for inclusion as they differed so slightly from those obtained with the discs. He presumed that the pressure effects of the rectangular plates would therefore also resemble those produced by the discs fairly closely. While on this question he might deal with Mr. Townend's remarks. Approximate calculations showed that while the drag of the annuli when in position on a body would be substantially reduced on account of the retarded flow in the boundary layer, the reduction so calculated was not enough to account entirely for the low drag of the annuli in the presence of the body. The effect suggested by Mr. Townend seemed quite possible; the drag coefficient of an annulus when in place on the body was probably considerably less than in free air.

Reverting to Mr. North's remarks, he was glad Mr. North had referred to the possible utilisation of the pressure distribution in conjunction with the local shape of the body to reduce drag. Although he (Mr. Ower) had mentioned this possibility in the paper, perhaps he had not emphasised it sufficiently; it seemed as if it might have an important bearing on design. But it should be noted that by playing about with the shape of the body at a given spot, the drag of the body itself might be changed, and any advantage due to the up-wind component of the local pressure drag might be outweighed by such drag changes unless care were taken.

Mr. North had said he would have liked to see the results of the body-wing interference tests carried out on combinations of the same over-all span rather than on wings projecting by a constant amount from the body surface. Work of this kind was now being done at the N.P.L., and a report would be issued in due course. The experiments described that evening had not been intended to be regarded as quantitative, but merely qualitative to show what wing positions were bad and what good. When the tests with complete wings came to be published, it would be seen that they confirmed the results obtained with the roots. In giving the results of these tests, drag variations at a given lift would be plotted, for as Mr. North had pointed out, this was the information the designer required. Mr. Ower had appreciated this all through his experiments and had adopted it whenever possible, but in the wing root experiments it could not be done as measurements were usually made at one incidence only in each wing position.

Probably quite a lot could be done to improve bad wing positions by bending the wing root as suggested by Mr. North. He (Mr. Ower) might conveniently refer at this stage to the question of the low-wing monoplane, which had been raised also by Mr. Hollis Williams and by Mr. Manning. Perhaps he had given rather an undeservedly bad name to this type of machine. He had not intended to imply that one could not produce such a monoplane to have a reasonably economical performance. What he had meant to convey was that if the body surface formed an acute angle with the upper wing surface, the combination would be a bad one, while with vertical-sided bodies the low-wing position was not nearly so bad, although it was probably not quite so good as higher positions. That would meet the point made by Mr. Manning concerning the Schneider Trophy machine; the angle at which the sides of the body met the wing was about 90°, and there was a good fairing at the root.

Mr. North had mentioned the question of interference in the neighbourhood of the empennage. Mr. Ower was not quite sure what Mr. North meant. If he was referring to the interaction between tail surfaces and the rear of the body, there seemed no reason why the general principles indicated in the paper should not apply. On the other hand, perhaps Mr. North had in mind the way in which the wake from the main planes affected the tail organs. That was an important question which was being studied at the N.P.L. In such a case the airscrew slipstream would be expected to have a large effect as Mr. Manning had pointed out, but it did not necessarily follow that the airscrew would have a similar effect in the types of interference discussed in the paper. He (Mr. Ower) would be surprised if the statement he had made to the effect that it was unlikely that the slipstream would convert a bad position into a good one proved to be untrue.

Mr. Relf's remarks on the possibility of inducing laminar flow to persist at high Reynolds numbers were encouraging. Possibly Mr. Manning had suggested the method by which the fish was able to do so, if the presumption that it was so able was in fact true. He entirely agreed with Mr. Relf as to the value of visualisation of flow; such experiments were not used to anything like the extent they should be. They could often be very simply carried out in the wind tunnel. Ordinary streamers of cotton attached to wires gave quite a lot of information and were frequently far preferable to a long series of lift and drag tests. Indeed, he suggested that the latter should always be preceded in interference researches by visual examination, and that one should only start measuring forces when the streamers indicated that a good combination had been arrived at.

Mr. Simmons' remarks were very interesting. The effect of dust was new to him and it was a point that would have to be watched in future by investigators carrying out tests on streamline bodies in the wind tunnel.

In reply to Mr. Perring, he hoped shortly to be able to test a few cases of body-wing interference in the presence of the slipstream, and when doing so would bear in mind Mr. Perring's remarks about fillets. While admitting that the transverse shape of the nose of the fuselage would probably have an effect on body-wing interference, he thought the shape of the nose in side elevation was more important.

Mr. Nayler's suggestion for rendering the flow visible was very interesting and seemed worth trying. It would, of course, be simpler to use in an enclosed return type of tunnel, but it did not seem quite impossible in the non-return type.

On the motion of the PRESIDENT, a hearty vote of thanks was accorded Mr. Ower for his paper.

FLIGHT IN NATURE AND IN SCIENCE

BY

R. GIACOMELLI, A.F.R.Ae.S.

The Conquest of the Air

Due proportions being established, it can be said that the present time, as regards what has been called the " conquest of the air," is comparable to that extremely remote epoch in which Nature for her part also solved the problem of flight.

Man arrived late in the history of his life at the realisation of locomotion in the air, but Nature also reached the same realisation only in a second stage of the evolution of living forms. The early animal species were all wingless, the development of the wings being the effect of a successive adaptation.

At present the number of fliers is very large.

From calculations of the zoologist Dödering, mentioned by the zoologist Zschokke, together with a large number of interesting data and information about animal flight in a very important book,[1] we know that among 420,000 animal species at present existing at least 260,000 are able to move through air. As is seen, flight is a faculty widespread in Nature; from the figures some 62 per cent. of the species living to-day is adapted to aerial locomotion. If from these calculations we exclude water creatures, which give a small contribution of fliers (and moreover of a very poor quality), the ratio of the flying to the non-flying species rises to 75 per cent.

Among 280,000 species of insects, 240,000 are capable of flight. To this army of flying species 13,000 species of birds and 600 species of bats have to be added.

Importance and Antiquity of Flight in Nature

We may see from this of what prime importance in the animal and vegetable economy of our planet the ability to fly is, and how deeply the conditions of life on the earth must have changed when flight developed in Nature. To the fliers all the earth is open and insects and birds were first to alight on the islands arisen from the sea and valleys formed on glaciers, where they were the pioneers of life. Let us consider also the enormous importance of flying insects for the pollination of flowers, that is, for conservation of such a great part of the vegetable world. The necessity of pollination establishes, in fact, the variety and charm of flowers, which have indeed their *raison d'être* in the flying life of insects.

Prior to insects having acquired the faculty of flying flowering plants (phanerogams) did not exist, and our earth was covered with forests of arboreous ferns and other flowerless plants (cryptogams). These fern forests on marshy ground, later buried by successive changes of the surface of our planet, formed our stores of coal, so that this period of the history of the earth which gave rise to them has been called the carboniferous period.

---

[1] F. Zschokke—" *Der Flug der Tiere.*" Berlin, Springer 1919. Another important work consulted is: " *How Insects Fly,*" by R. E. Snodgrass (Smithsonian Report, Washington, 1930).

In that extremely remote epoch the date of which has been established as about three hundred million years ago, in which mammals, birds, and flowering plants had not yet appeared on the earth, the problem of flight had been already solved by Nature in two kinds of creatures—in insects and in some flying reptiles which then were living.

Ancient and Modern Fliers

Notwithstanding the remoteness of the epoch the insects of the carboniferous period, as is evidenced by their fossil remains, did not differ essentially from the present ones; they belonged indeed for the main part to beetles, dragon flies, and a group now extinct (paleodictyoptera).

All these insects were fitted with two pairs of well-developed wings, the structure of which is so like that of the wings of present-day insects that only a specialist could discover the difference by a close examination of the venation.

The characteristic insect structure in three parts, the head, the thorax of three segments (of which the second and the third are each fitted with a pair of wings, and all three are fitted each with a pair of legs), and the abdomen was already definitely fixed.

On the contrary the flying reptiles, or pterosauria, the contemporaries of insects in the earliest natural solution of the flight problem, no longer exist, having disappeared in a very early epoch of the life of our planet without leaving descendants. We cannot, indeed, take for their remote descendant the flying dragon of the island of Sumatra, which is a small lizard which, being fitted at the sides of its body with a membrane spread by the long ribs of its thorax (which do not close in front), uses the same membrane as a parachute for jumping from the tops of the trees to the ground or from branch to branch.

When the pterosauria became extinct they had reached a high degree of development, and yet Nature at a given moment gave up further advancement in this direction. The results of a very long evolution were lost, and Nature began again to try solving the flight problem in another way, by creating two new types of flying vertebrata, essentially different from the former—bats and birds.

As is seen Nature attempted to solve the flight problem along various lines, and not alone this, but it did not hesitate to abandon one way to try others when the former, whether in itself or through changed circumstances, seemed no more suitable.

Science also is trying to solve the flight problem in more than one way; as well as on the main solution, that is to say, the aeroplane, it is working on the helicopter and the autogiro, while there are also those who still believe possible that solution of which man thought first, the realisation of the flapping wing machine like birds—the ornithopter.

Statical and Dynamical Sustentation in Air and Water

But science also found a solution of the flight problem which had not been tried by Nature—to fly by sustentation based on the statical principle or of buoyancy, whereas all natural fliers base their sustentation in flight on the dynamical principle, that is, on the existence of a relative velocity between the air and the wings.

The principle of statical sustentation, however, is not unknown to Nature, as it is applied by the latter to the locomotion of creatures in water; a fish, if it stops the motion of its tail (which is its driving organ while its fins are an organ of direction only) does not sink, as does a bird if it closes its wings. But Nature has not applied the statical principle to the locomotion of creatures in air, exactly as it did not apply the dynamical principle to locomotion of creatures in water. Would it have been difficult for Nature to develop on the back of the

creature, destined to fly, a bag of suitable size, filled with a gaseous substance lighter than air, or to transform the extremities of other creatures, destined to swim, into small fins gliding on water? These are not impossible solutions, but questions of efficiency and suitability must have guided Nature in using both these principles, by limiting the statical principle to water, that is to say, to the denser fluid, and the dynamical principle to air, the more rarefied.

On the contrary, science, as it solved with the airship in air the same problem as in water with the ship, so it solved with the hydroplane in water the same problem as in air with the aeroplane.

The question now arises if science with this pair of double solutions has succeeded in obtaining valuable results, and if the solution, in disagreement with Nature, is as efficient and suitable as the natural model. In other words, are the airship and hydroplane able to emulate respectively the aeroplane and the ship?

As to the hydroplane (a machine very closely studied by General Crocco some twenty-five years ago)[2] its practical development has not yet satisfied the hopes which had been originally founded on it; which, however, does not exclude that for particular and restricted purposes a future may not be anticipated for it.

As to the airship, after a brilliant past in which it succeeded in realising aerial locomotion prior to the aeroplane, and after having clearly shown the way of its development in the increase (up to a very high degree) of dimensions, we are in a state of uncertainty, not about the possibility of its further development, but about its suitability. We are dubious indeed as to the suitability of persisting any longer in this line, owing to the progress attained in the meantime by the aeroplane, which was not anticipated, but rather was theoretically excluded when the airship technique was founded. Experience at any rate continues and it only can decide if the way followed up to the present is right or wrong. But the airship experiment is expensive and not all countries are prepared to meet it. France even, which conceived the airship, Italy which profoundly contributed to its development with the semi-rigid type, and England which in the post-war period promoted a new technique, have successively retired. Experiments however continue in Germany and in the United States, and the results which will be finally obtained will show whether science will succeed in doing what was not done by Nature.

Origin of the Wing of Insects

The flying creatures of to-day originated from wingless creatures, apterous ancestors. Science has tried to reconstruct the genesis of fliers from their non-flying progenitors, but for the present the reconstruction is only hypothetical, although based on very plausible conjectures, as there has not yet been found in the fossil remains, evidence of the primitive creatures.

We will touch briefly on this conjectural reconstruction starting from insects. This reconstruction, like others of this kind, is made by taking into consideration at the same time both the fossil remains (paleontology) and the present development of the creature (ontology).

If we observe a modern insect in its adult stage we see that its wings constitute flat outgrowths of the lateral edges of the back plates of the second and third segments of its thorax. But insects are not born with wings; the latter develop later, beginning as flat pads or pouch-like outgrowths of the body wall, the two larger of which successively come together, and form the upper and lower surfaces of the mature wings, while the canals of the veins represent the remnants of the original cavity.

[2] Crocco's hydroplane, constructed in the year 1907, was on view during the International Aero Exhibition of the year 1929, at the *Science Museum*, South Kensington.

By observing, on the other hand, carboniferous insects we establish, in several species, the existence of a pair of small flat lobes, resembling the developing wing pads, which extend laterally from the margin of the first thoracic segment (where insects do not bear wings); which lobes do not appear in modern insects.

This leads to the very probable hypothesis that these small lateral flat lobes have begun to form on the back of the primitive insect on all three segments of the thorax, and that only these having been formed on the second and third segment continued their development, giving rise to the wings, while those on the first segment, after having remained for a certain time, as they had no more function to complete, ended by disappearing.

What must have been the function for which these hypothetical six lobes of the primitive insect could have served? Without doubt a gliding flight function, allowing the creature to descend from elevated positions to the ground or from one branch to another in a more or less flat path. Gliding flight was transformed into true flight when, with the successive development of the lobes on the second and third segment of the thorax, these became true wings, while the first two lobes atrophied.

Not all insects acquired the capacity to fly. There are to-day some small insects known as apterigota, in which not only do we find wings completely lacking, but there is not found the slightest evidence that these creatures are descended from winged ancestors. On the contrary, in other insects we find traces of the successive loss of wings which, for instance, in some insects appear only for a short period of their life, as in termites and ants. Therefore apterigota are apparently direct descendants of the supposed primitive wingless insects.

The Wing in Vertebrata and Invertebrata

If we compare the wing formation in vertebrata with that of the invertebrata now examined we find that this formation rests in these two types on a quite different principle. In fact, while in insects Nature evolved on the back of the creature two lobes without diminishing in general the organs of locomotion on land or water which it enjoyed, in the case of vertebrata Nature adapted to the new function organs already existing. Thus, while butterflies, flies, and insects generally conquered the air without any sacrifice, vertebrata obtained that conquest at a great cost. The wings of birds, of bats, and of pterosauria are a profound transformation of the front member, which, in acquiring its new function, lost its original one. Nature, therefore, followed two ways to give rise to the wing, according as it was needed to give the power of flight either to an invertebrate or a vertebrate creature, respectively, by the addition of new parts or the transformation of those already existing.

In the wing formation, both for invertebrata and vertebrata, there are however some main principles which Nature always observed. The articulation of the wing lies always before or above the centre of gravity of the creature, and the leading edge of the wing becomes rigid (in insects through a strong vein, in vertebrata through the bone of the arm suitably transformed), while the trailing edge remains elastic.

Requirements of Flight

With the formation of wings the creature underwent considerable alterations in its structure. Flight called for high requirements and moulded the body of the creature down to the smallest details; it required more compact muscles, lighter bones, broader respiratory organs, and keener sight. Flight does not inevitably compel the renunciation of walking, climbing, and swimming. In fact, ducks and swans swim, and woodpeckers climb. Others instead, such as dragon-flies, butterflies, bats, and pterosauria, almost totally lost the ability to

walk, becoming flying machines with unilateral specialisation, so that the ground became for them only a place to rest, hatch, or from which to take food. However, with this too advanced specialisation the flying creature meets the fate of all creatures excessively specialised; the harmony of structure and the balance of faculties suffer.

Another requirement of flight is a rather low limit of dimension. In birds the ostrich exceeded the size suitable for flight, so that secondarily it lost this ability, and some gigantic wingless grasshoppers appear to have exceeded the assigned limits. In aviation certain rather low limits of dimensions have been put forward. These limits, with the progress of the science, have been steadily increased, so that to-day we have an aerial colossus like the Do. X; but this point having been reached the question rises again if what is possible is at the same time suitable.

It is certain that with the fantastic group of flying reptiles Nature was able to reach much greater dimensions than with present fliers. Among the sixty species which have been reckoned among pterosauria, while there are not lacking specimens of the size of a sparrow (pterodactyles), there are also others (pteronodontes) in which a span of thirty feet was reached. The condor of the Andes, the giant of present fliers, has a wing-span of only nine feet.

The hypothesis has been put forward, to explain the huge size of former flying creatures, of the existence at those epochs of an atmosphere denser than the present.

History of the Pterosauria

We know that at a culminating point of their development they suddenly disappeared. The earliest and most primitive species, of which vestiges are found in the fossil remains, possessed a pair of well developed wings. A featherless membrane (patagium) of triangular form, tapering like the tail of a swallow, ran along each side of its body from the arm down to the leg, excluding the foot and the hand, except the fifth finger was enormously developed in length and included in the wing, which was kept extended by this finger, constituting by prolongation of the forearm its leading edge. All the other fingers and all the toes were short and fitted with strong talons. These early pterosauria had teeth and a tail.

Successively the flying reptile changes, adapting itself more and more to flight. The patagium became narrower and longer, with which process the fifth finger, keeping it extended, had to get longer and longer, the tail diminished, and the teeth disappeared, while the spinal column stiffened.

The representative of this second step in the evolution of flying reptiles is the ramphorincus, belonging to the geological period known as the Giura. Fitted with a tail carrying at its end an horizontal membranaceous plate (probably a balance organ) this creature represented the oddest form of flying animal which ever existed.

A short time before the ramphorincus died out, in the lower layers of the cretaceous stratum, there are found vestiges of other species having in the place of teeth a beak, probably horny. In these species the successive adaptation to the function of flight led to a further stiffening of the spinal column, by welding together the vertebræ of the os sacrum with the preceding dorsal vertebræ. The bones are lightened through the formation of cavities (pneumatised bones), a keeled projection on the sternum forms, and finally warm blood circulation was reached; all characteristics which Nature repeated then in the birds. To this more evolved type of flying reptiles belong the pterodonta, remnants of which have been found in America, representative of the giants of the class, with a twenty-seven feet span and 269 sq. ft. of wing surface.

They flew in the Gulf of Mexico hunting the flying fish, and, notwithstanding their great dimensions, the creature was light. Its lower extremities were poorly developed and served only to sustain the wing membrane, so that it was quite unfit to walk, and when on the ground it lay on its face on the rocks, along which it drew itself by climbing with its talons in order to reach a height from which to take flight. It was a flying machine constituted by a rigid frame of pneumatised bones, and a skin with little flesh. Zoologists agree in considering it as the most specialised representative of the flight function among the flying organisms of all time. They think also that in this extreme adaptation to the function of flight there is to be found the cause itself of its disappearance, as the way of the adaptation of its body to flight, followed up to its extreme limit, reduced it to a blind alley in which there was no longer possible either progress or retrogression.

The flying reptiles died out at the end of the cretaceous period. Their heirs were bats and birds; heirs from the point of view of the function of flight, not from that of descent, for they attained flight in other ways.

Adaptation to Flight in the Bats

The wing membrane of the bats or cheiropters is attached to the arm, and the hand, four fingers of which developed an extraordinary length and served to extend the wing, without having any other function. Only the thumb remained small and outside of the wing, maintained its talon, and is substituted for the whole hand in climbing. The wing membrane reaches along the sides of the body, the legs and the tail, which becomes a complete rudder in insectivorous bats, which hunt their prey along a tortuous path. This rudder is lacking in frugivorous bats (the red bat of North America). The thorax is rigid and the osseous frame is light. The lightness is not obtained as in the most developed pterosauria and in the birds by means of aerial cavities. A strong thoracic girdle gives support to the flight members and the sternum is usually keeled.

The wing membrane of bats has a very great area and mobility in comparison with the simple parachute of the early flying creatures. Bats are in fact experienced fliers. They are clumsy and awkward on flat ground, on which they move by clinging with their taloned thumb, while the talons of the legs serve to hang to the clefts and to the branches head downwards, in order to be able, at the moment of taking flight, to stretch the wings easily.

The power of flying varies among the 600 species constituting the class, according to the form of the wings. The species with very long and narrow wings are strong fliers, remaining in the air for many hours and reaching great heights. They are not afraid of storms and are able to carry out short soaring flights. The species with wide and short wings are inefficient fliers. These latter represent the most primitive types, while the former represent a successive step in the adaptation of the creature to flight.

Adaptation to Flight in the Birds

The masterpiece of Nature in the adaptation of vertebrata to flight is undoubtedly the bird, whose excellence in flight is produced by two factors, the wing shape and the adaptation of the body down to the smallest particulars to the fluid medium, an adaptation not yet made by the modern aeroplane. With regard to the latter Professor Melvill Jones has calculated that one-third of the horse-power only should be necessary if the adaptation of their form to flight was that of the albatross.[3]

The bone of the arm and the hand is transformed in birds to constitute the flight organ more deeply and harmoniously than in pterosauria and cheiroptera,

[3] "*The Streamline Aeroplane*," by Melvill Jones. The Journal of the Royal Aeronautical Society, XXXIII., 1929, May, pp. 357-385.

while their legs remain quite free, continuing their function of supporting the body on the ground and on the branches.

Flight, however, imposes its requirements on all the osseous frame; the arm and hand become deprived of the function of taking food and of offensive and defensive action, while the neck is lengthened highly to permit the mouth to approach the ground. The neck carries a strong and light cranium on which a horny beak is substituted for the heavy teeth of its predecessors. The vertebral column is shortened, being merged with the last vertebræ to give strong support to the tail. The thorax becomes rigid to constitute a powerful base for the work of the wings. The articulation of the shoulders, to which is connected the relatively short and strong humerus, ensures free movement to the wing, constituted by a long forearm. The fingers are reduced to three as the fourth and the fifth have disappeared. Among these three the thumb is reduced to a stump on which is set a tuft of feathers, the so-called " alula " (a characteristic organ on the function of which very complete observations were made by Leonardo da Vinci, and in recent times by Mr. A. P. Thurston), while the middle and forefinger are combined in one, and lengthened, without attaining however the great length of the one finger which holds extended the wing of the pterosauria, or the length of the four wing-fingers of the cheiroptera. Their restricted length is however balanced by the remiges which are attached to these two fingers, obtaining by this means a better functional result than that obtained from either of the other types of creature with the excessive direct prolongation of the fingers.

The muscular system reaches a special power in birds, acquires high capacity of contraction, and forms on the breast of the creature a powerful mass which finds support on the great and robust sternum. The principle of the lightening of the weight is carried to a high degree by means of the pneumatisation of the bones and the disappearance of the heavy marrow, replaced by a void space, and finally through formation in the thoracic cavity of aerial bags.

The powerful work of flight requires a correspondingly efficient circulatory and blood purifying system, while not to burden the body excessively a rapid digestion is required. Moreover, the body cavity is full and compact, so as not to give rise to shocks and displacements. Characteristic, finally, is the continuous mobility of birds, which has its explanation in the continuous necessity for finding food, because, as has been observed by Brehm, it must not be forgotten that " the always hungry stomach, owing to the rapid digestion, leaves never in rest this warm-blooded master of motion."

From the Climbers to the Fliers

Whence do bats and birds derive? For the former it has been possible to establish, through the comparative investigation of fossil remains and present creatures, that they developed in a relatively short period from arboreal creatures, likewise mammals, and that in the Eocene period the air was thick with bats.

For birds it has been similarly possible to establish that they are derived from arboreal creatures, and more particularly from reptiles, as was the case with the pterosauria. Birds have been traced back, in their fossil remains, up to the geological period known as the Giura, but the types living at that epoch were quite different from present birds; they were a kind of intermediate creature between the reptile and the bird.

Birds and bats proceed from arboreal creatures, that is to say, from climbing creatures, as did the pterosauria, so that the conclusion is that the flight of vertebrata has had origin in the wood.

This conclusion follows from the recognition of the survival in their descendants of well-nailed extremities, such as were necessary to their ancestors for climbing trees.

As for pterosauria, it has already been pointed out how their fingers and toes were fitted with strong talons, through which the creature, having become quite incapable of walking, dragged itself along the ground, by clinging to the rocks. Like the bats, extremely clumsy and embarrassed while on the ground, they drew themselves along the latter by clinging with the taloned thumb, the only finger which the function of flight had left free, while with the talons of the feet they hung head downward from the projections of the rocks or from the branches.

Birds of to-day have lost the talons of the hand and with them the faculty of climbing. The woodpecker and the parrot climb, but clinging, owing to the lack of the talons of the hand, by their beak. In some different way, probably, the supposed progenitor of present birds, that intermediate creature between the reptile and the bird, the *archeopterix macrura*, was able to climb.

The Archeopterix and the Opisthocomus

The archeopterix or the primitive bird, still possessed from the reptile, jaws fitted with teeth, a sternum lacking in the keel, hands and the feet with fingers and toes well taloned, and a long tail, but possessed the feathered wing of the bird, although incomplete, and a feathered tail.

The wing lacking in the remiges, which are essential for propulsion in flight, was formed by a simple row of feathers set on the forearm; the hand was completely free for the function of climbing. The tail carried on both sides a row of feathers. The creature therefore was not in a position to make a real flight, but was able only, in jumping downward, to brake the descent by spreading the forearm and tail feathers like a parachute, and to carry out short gliding flights. To get up again it climbed, making use of its powerful talons. The successive development of the wing, with inclusion of all the hand, allowed the creature to pass on gradually from simple parachuting and gliding to perfect flight. Whereas through the disappearance of the hand talons the creature lost the faculty of climbing, the loss was balanced to a great extent by the acquisition of the art of flying.

There is a singular case in which the taloned hand and the climbing function appear again to-day to give evidence of the origin of birds. This case is presented by a bird of Central America, the opisthocomus, which, while a fledgling, carries at the end of its wings two talons set on its middle forefinger with which it climbs on the trees. As the adult stage is reached the two talons disappear, and the opisthocomus, which for a short period of its life has been a climber, like its remote ancestors, becomes exclusively a flier. All birds are fliers or have been such, for though some of them fly no longer, as the ostrich or the penguins, this is the result of a successive involution, as they all descended from flying progenitors.

The Wing Covering of Flying Vertebrata

A question discussed among zoologists is whether it is possible to establish a common origin between the wing covering of the bats and the pterosauria and that of birds; in other words, between patagium and feathers.

The explanation of the origin of the membrane wing does not present any difficulty; it is a case of a membrane formed at first between the anterior member and the trunk, and subsequently developed with total or partial inclusion of this member suitably transformed to constitute the frame of the wing.

Can feathers be considered as a formation derived secondarily from an original membrane? Neither paleontology nor ontology have yet supplied reasons to show that birds are derived from creatures fitted originally with a patagium.

The origin of flying vertebrata, therefore, is similar for all, on account of the fact that all are descended from clinging and arboreal predecessors, which

at first became parachutists and gliders, and subsequently fliers; but in the formation of the primitive parachute, which then developed into the wing, Nature in pterosauria and bats used always membraneous material, while in birds instead it must have used the feather from the very beginning.

The wing of the pterosaurius and of the bat, on the one hand, and that of the bird on the other, differ in other essential characteristics, which have influence on the creature's method of life. In fact the bones of the wing of the pterosaurius and the bat were constructed at the expense of one part of the hand only, while that of the bird was constructed at the expense of all the hand. To the former kind of creatures there thus remained, with that part of the hand which had not been included in the wing, the ability to climb, which was lost for birds. In the membraneous wing the legs remained imprisoned, so that pterosauria and bats were compelled to renounce more or less completely the function of walking. In the formation of the feathered wing the legs remained completely outside, putting birds in a privileged condition in comparison with the other two in the dominion of the air.

Minor Fliers in Animal and Vegetable Kingdoms

To these kingdoms belong some living animal species incapable of flight properly so-called, that is to say, with horizontal and ascending progression, but capable only of braked descents or at the most of gliding flight.

Of these creatures repeating a stage passed by the present perfect fliers, there are some examples in the lizard of Sumatra, in which its ribs do not close on the sternum but remain open to the side of the spinal column, keeping extended a membrane by means of which the creature brakes its descents while jumping from the branches to the ground.

With a lateral membrane is also fitted a small toad inhabitant of the forest of the same islands of the Sound, the racophorus. These islands are inhabited also by the galeopithecus (a creature much discussed, which first was classified among lemurides and now among insectivori), while in Siberia there exists a squirrel in which, as in the galeopithecus, the skin extends at the sides of the body between the front and back member, for the purpose of braking jumps downward.

Other examples of such parachutists and gliders are supplied by the so-called flying fish, and in the vegetable kingdom by some kinds of flying seeds.

The flying fish jumps in the air and glides with the use of large fins. The flying fish in this way escapes from the fish which pursue it in the water, but often during its path in the air it becomes the victim of the sea birds.

The winged seeds have two membraneous lobes giving a slower rate of descent from the trees than for ordinary seeds and so, with the aid of the wind, ensuring a wider dissemination. The maple, the lime, etc., have such winged seeds. A characteristic parachuting formation is shared by the seeds of same compositæ in which an umbrella-shaped tuft of hairs allows them to soar for a long time on the wind. But the most characteristic specimen of winged seed is supplied by the famous zanonia, a cucurbitaceous plant of the islands of the Sound (where besides the flying dragon, the flying toad and the galeopithecus there is found this other aeronautical curiosity). The zanonia seed is so well designed with its membraneous lobes and so well balanced and stable in gliding that it was taken as a model by the German constructor Dietrich in order to develop a very stable wing.

This wing has been adopted on the well-known " Tauben " aeroplanes of the war period, and is still used successfully on the present commercial German " Focke Wulf " aeroplanes.

The Origin of the Airship and of the Aeroplane

On the earliest conceptions of both these machines enough has been written without detailing either the first design of an airship by the French Lieutenant Meusnier in the year 1783 (the same year in which the brothers Montgolfier launched their balloon in the air), or about the work of Sir George Cayley for his discovery of the principle of the aeroplane in the year 1809. His famous resolution of the "*whole force under the wing into two forces, the former representing the force that sustains the weight of the bird, the latter the retarding force,*" is in fact well known, as well as his no less famous statement of "*the whole problem of flight within these limits; to make a surface support a given weight by the application of power to the resistance of air.*"[4]

Similarly it is no longer necessary to expatiate on the history of those[5] who, in the footsteps of Cayley, carried forward the solution of the great problem of enabling man to fly—Henson the promoter of the "first aeroplane project" in 1842, and his collaborator Stringfellow who showed in 1848 that an aeroplane model could fly under its own power. This demonstration was repeated to a greater extent in the year 1871 in France, by Alphonse Penaud, who exhumed the forgotten paper of Cayley, thus vindicating his priority in the invention of the aeroplane, and some years after, in the year 1878, again in France, by Victor Tatin, and in Austria by Wilhelm Kress (1877). The history of the latter being less well known deserves to be briefly touched on. Kress lived in Vienna and from 1864 onwards occupied himself with aerial screws with the purpose of applying them to a project for a helicopter, and one day of that year, as he himself told in his book, "*How the Bird Flies, How Man Shall Fly*"[6] (1905), he amused himself by launching a boy's kite. On account of the lack of wind he was forced to run fast to make the kite rise. It occurred to him that the problem would be solved by fitting a screw to the kite and driven by a suitable motor. Kress reached thus unexpectedly the conception of the aeroplane. After several years of work he succeeded in making a model fly in the year 1877.

The fates of the man-carrying machine by Hiram Maxim, in 1894; of the machine by the American scientist Pierpont Langley; and finally the dramatic history of the great French pioneer Clement Ader who, after forty years of labour, saw his "*Avion*" smashed up on the ground after some hops without achieving an actual flight, demonstrated that full-sized power-driven aeroplanes were not yet possible, though flying models could be made.

The Actual Realisation of the Aeroplane and of the Airship

We have thus arrived at the famous experiments carried out in Germany by Otto Lilienthal with the assistance of his brother Gustavus. Their gliding experiments carried out at a low height, gradually put man in a position to become familiar with the atmosphere, and with the manœuvres necessary for balance, giving him at the same time the possibility of checking the actual movements of the machine in the air, and of applying to the glider all improvements suggested by experience.

This sound and reasonable method of experimenting, introduced by Captain Ferber in France and by Chanute in America, caused the latter to state that he

---

[4] "*Aerial Navigation,*" by Sir George Cayley, 1809. The Aeronautical Classics. Edited for the Council of the Aeronautical Society of Great Britain, 1910, p. 6.

[5] "*The History of Aeronautics in Great Britain,*" by J. E. Hodgson. London, 1924.
"*Flying,*" by Lieut.-Col. W. Lockwood Marsh. London, 1929.
"*Handbook of the Collections Illustrating Heavier-than-Air Craft,*" by M. J. B. Davy, Science Museum, 1929.
"*Henson and Stringfellow—Their Work in Aeronautics.*" By M. J. B. Davy. Science Museum, London, 1931.

[6] "*Aviatik, wie der Vogel fliegt und wie der Mensch fliegen wird,*" by W. Kress. Vienna, 1905—French Translation in the year 1909.

had learned more by this way in three weeks than in twenty years of tests on models and of laboratory researches. Chanute taught this method to the brothers Wright, who mastered the machine in flight and fitted it with their great invention, the organ of lateral stability, and on that memorable December 17th, 1903, succeeded in carrying out the first motor-driven controlled flight.

As to the evolution of the airship, it can be said that from the rudimentary project of Meusnier to reach an actually efficient machine, it is necessary to omit all the numerous intermediate attempts up to the studies of Captain Renard in France and his dirigible constructed in the year 1883; studies continued by Count Zeppelin in Germany, whose rigid airship carried out its first flight in the year 1900, and by General Crocco and by E. Forlanini in Italy, whose semi-rigid airships flew in the years 1908 and 1909.

To close this summary of the origin of the aeroplane and airship let us give an account of their names. For the former there were used successively various names; at first simply flying machine or other analogous general expressions. Then Penaud named it *planophorus* as bearing a plane, the plane of the wings, Kress *aeroveloce*, Langley *aerodrome*, that is to say, *air runner*, and Ader *avion*, a name which has remained in French. To-day it is called *aeroplane*, although this word, of English origin, signified at first only air plane, the modern *aerofoil*. From indicating a wing it subsequently came to indicate the apparatus bearing the wing, just as aerodrome from its primitive meaning went to indicate much more suitably *flying field*.

As for the term dirigible, the word which in origin was an adjective became then a substantive. At first the phrase "*dirigible balloon*" was used, and by shortening it became "dirigible." Finally now the term dirigible has been in this country generally replaced by that of *airship*. Such are the vicissitudes of words!

The Method of the Brothers Lilienthal and the Realisation of the Brothers Wright

The problem of motor aviation was in practice solved by science only after having passed through the obligatory way of gliding flight, and in this he followed the way that had been followed by Nature so many millions of years before.

It must not be forgotten, however, that all the motor flying machines, preceding that of the brothers Wright, failed to leave the ground principally for the lack of an adequate motor. On the other hand, it is also easy to see what would have been the fate of a machine which, although fitted with a sufficient power, had not passed through the course of more than ten years of gliding experiments made by the brothers Lilienthal and Wright, and guided by a man quite ignorant of air.

The factors for success were two: On the one hand a suitable method for obtaining control of the machine and its practical adaptation to the air, and on the other hand a sufficient motive power. Circumstances put at the disposal of the brothers Wright both these factors; but no less indispensable for the highest efficiency of these latter was the great certainty of success of the two brothers, combined with a true scientific spirit and a very great mechanical ability.

Otto Lilienthal, a very modest and sincere man, did not glory in the fact of having, by his own method, put humanity on the same road as that followed by Nature for solving the flight problem, as he did not boast of having found it by reflection or at least of its being revealed to him by direct observation of Nature. On the contrary, his brother Gustavus, his faithful collaborator, has told in a recent book of his[7] that they found their method by chance, and that only after having obtained their first success did they become aware of having discovered the road already followed by Nature.

[7] Gustav Lilienthal *Die Biotechnik des Fliegens*. Verlag in Leipzig R. Voigtländers 1925 (pages 97-98).

The chance, as Gustavus Lilienthal tells, occurred as follows. They were investigating the superiority of cambered plates over flat ones, with regard to increase of lift and reduction of head resistance, and the limited means at their disposal did not allow them to carry out full-scale experiments on the reaction of air with suitable equipment. They were then constrained to load these surfaces with the weight of their own bodies, and to expose them so loaded to the wind. The first of these experiments was carried out by using a cambered section surface with sharp lateral tips, a chord of three feet, a span of twelve feet, and having in the middle a simple opening through which passed the body of the experimenter.

After the first attempts in which, owing to poor equilibrium, Gustavus tumbled on the sand, both brothers made some alterations in the surfaces until they took the form of cambered wings which the brothers verified had small head resistance and great lift. "*Those wings sustained man in air!*" The flying machine was created. It was indeed very primitive as it consisted only of these wings, which extended to the right and left of the body of the experimenter, the latter being supported on his elbows and shoulders, while his feet were below. The wings, moreover, were somewhat turned up (that is to say, they had a slight upward dihedral) on account of the equilibrium, which Otto Lilienthal maintained by moving his weight in relation to the machine. Two rudders, one vertical and the other horizontal, subsequently completed the machine.

Up to the day of his death, August 8th, 1896, Otto Lilienthal carried out, as is well known, about 2,000 glides; in the last two years starting from an artificial hill 50ft. high at Lichterfeld, near Berlin.

Why Flight Has Been Realised So Late

The importance of Otto Lilienthal's glides was not understood in his lifetime, neither in Germany, his native country, nor in France, which at that time was the centre of aviation experiments. To his glides the epithet of flight was denied, as they were considered only as simple braked falls and exercises of agility, owing to the continuous and fatiguing motions of the body to which he was constrained to maintain the equilibrium of the machine, so that in France he was qualified as a mere *parachutist*, and in Germany no more than an *acrobat*.

But when the realisation of motor flight by the brothers Wright followed, and it was seen that those easy exercises of gliding flight had essentially served in the rise of aviation, the question arose why so easy a matter, and carried out by Lilienthal with such primitive means, had not been realised for so many centuries. It is the same question which could be asked as to why the hot air balloon of the brothers Montgolfier, as simple and realisable with such primitive means, had not been invented long before.

The reason is found in the very fact that the idea of realising flight did not begin seriously to attract attention until late, except from time to time by the usual "*flying lunatic*," and was not in any way due to an inherent difficulty in the problem.

At any rate, even the theoretical conception of the aeroplane was supplied without a great effort by Cayley, who, though undoubtedly a man of a superior mind and of deep mechanical insight, was certainly neither a Galileo, nor a Newton, nor a D'Alembert, nor a Lagrange, nor Helmholtz,[8] all of whom brought no contribution to the flight problem for the single reason that they had never considered it. The same can be said of the theoretical conception of the balloon by Father Lana, and by the brothers Montgolfier, as well as that of the airship by

[8] Truly Helmholtz in a way dealt with flight and precisely to demonstrate that man could never fly with the forces of the muscular energy. (Berlin Akad. Monatsberichten, 1873, p. 501).

Meusnier, who were also very far from any comparison with such giants of mechanics.

The Presumed Aspiration of Man to Fly

On the other hand, we cannot deny that until recent years it was not possible for anyone to devote himself to the flight problem, particularly to its realisation through the heavier-than-air craft, without incurring the charge of lunacy. And this for two reasons: First, because flight seemed generally a fact beyond human nature, reserved to birds, angels, and devils; and second because, owing to the special charm which things considered quite impossible and inaccessible by the average man exercise on brains not too well balanced, many lunatics in all ages so much excited their imaginations that, by attaching to their shoulders a pair of wings, they believed themselves transformed at once into birds, and jumped either from a tower or a bridge. They thus created that comic figure, called the " flying man," which gave rise to many caricatures, and supplied laughing matter for several generations. It must not be forgotten also that up to two centuries ago, in making flight experiments there was the danger of being taken for a sorcerer, exactly as happened to the Brazilian Father Bartholomy De Gusmao, who, in the year 1724, in Portugal, on account of his aerostatic experiments with which he forestalled the brothers Montgolfier, was arrested and brought before the Tribunal of the Inquisition on the charge of sorcery, and his writings burned. Such ideas were the last remnants of a widespread belief for centuries in the existence of a "*secreta ars volandi*" by magic virtue, on which there had been much discussion and writings by ecclesiastical and profane authors.

Finally it is enough to remember that Mr. F. W. Lanchester, the founder of the modern theory of sustentation in flight, and who thirty-nine years ago, as a young man of twenty-five, delivered a lecture on the matter before the *Birmingham Natural History and Philosophical Society*, was looked upon as being mad.

"*In* 1894 "—according to Lanchester's own words in a letter of last year to the author—"*one was considered mad to dabble in the problem of flight. I remember after my paper my friend Dugald Clerk taking me seriously to task on mad account.*"

Therefore, when we speak of a constant aspiration of humanity to fly, we only repeat an idle fable. On the contrary, it must be said that with the exception of few privileged intellects who, like the great and unfortunate Louis Mouillard, had in their blood "*la terrible maladie; le virus de l'amour de l'aviation*," flying has always been considered inaccessible to man and the thought of it folly.

The brothers Wright related that of the many people invited to attend their first motor flight, on the morning of December 17th, 1903, very few came, and these only with the idea of seeing "*a flying machine which would not fly.*"

In the year 1908 Delagrange carried out his first aeroplane exhibitions in Rome, on the first day (May 23rd) actually without rising over the ground. On the days following he rose fully to nine feet, and the majority of the public, who had not seen and did not want to see it, denied *a priori* the fact. In the advertisements there was the sentence "*Delagrange will fly,*" and this sentence became a saying, which the author often heard in Rome until ten or twelve years ago, to answer ironically a promise which neither will nor can be maintained.

From Gliding to Soaring Flight

The supreme ideal of Otto Lilienthal in the execution of his glides was that of being one day able to rise above his starting point, that is to say, to pass on to soaring flight, by imitating the storks which were his favourite birds, as the kite was the favourite bird of Leonardo da Vinci, and the vulture that of

Mouillard. But he was never able to succeed in soaring, a failure which gave him great discouragement.

Mouillard continuously tried to carry out soaring flight, but not having followed, like Lilienthal, a practical apprenticeship, he was never able to carry out even gliding flight. He built machines and many times tried facing the wind to carry out a flight, but each time fell and broke the machine. He ceased finally to make experiments which the daily occupation of his unhappy life did not allow him to carry out systematically. He continued all his life, however, to observe the flight of birds, and to write on natural and mechanical flight much of profound truth.[9]

Lilienthal's difficulty in attaining soaring flight, which is making new progress every year, depended on his erroneous ideas of the nature of this kind of flight. It appears extraordinarily simple to-day, but the explanation of soaring flight was, for a long period in aeronautics or rather in physics and mechanics, a puzzle of which no solution could be found.

The Early Hypotheses on Soaring Flight

It would be too long and even uninteresting to set forth all of these. The first hypothesis was that of denying *a priori* the existence of the phenomenon, by attributing it to an illusion, so that Mouillard strove in vain in his writings to explain that the phenomenon was quite true and always visible to "*those who wished to see.*"

The reason of the denial of the phenomenon depended principally on the consideration that a presumed flight without any expenditure of energy on the part of birds, as a flight with motionless wings seemed a mechanical absurdity to be classified together with that of the "*perpetuum mobile.*"

With regard to those who admitted this phenomenon, some attributed it to vibratory motions of the wing tips, imperceptible to the sight, but enough to produce a sufficient lifting action of the air; others to the interposition of rarefied and hot air particles among the feathers, lightening the body of the bird, which so became a sort of balloon; others to a mysterious phenomenon of *levitation* (an oddness still admitted fifty years ago); others to the pure and simple power of the wind, considered as a horizontal and uniform current, thus comparing the bird with motionless and spread wings to a boy's kite; and finally others (these only rightly) either to the existence of gusts and irregular motions of the wind, utilised by the bird suitably steering its wings, or to the presence of ascending currents of air.

Leaving apart the first hypothesis let us consider the third last, that of the power of the wind, considered as a uniform and horizontal current. This hypothesis was the effect of a faulty widespread conception current up to a few years ago. This error consisted in not understanding that wind exercises its force on us inasmuch as we are standing with our feet on the ground, while if we rise into air we do not feel that resistance any more, that is to say, the resistance does not exist as long as we do not assume motion relative to the air. Lilienthal did not share this error. In practice, however, he came near admitting something like it, as he believed that a wind, even regular and uniform, meeting with a cambered surface, of the type of the wing of a bird, free in the air, would be able to support it.

To understand the reason of his opinion it is necessary to state that he believed he had discovered that any wind whatever possessed an upward component, which therefore could be caught by a wing of suitable form and used

[9] Louis Mouillard "*L'Empire de l'Air.*" Paris, 1881. "*Le vol sans battement*" ouvrage posthume. Paris, 1912.

for sustentation. In this suitable form of the wing "*presumably rests*" (in his own words) "*the secret of the whole art of flying.*"[10]

Soaring and Flapping Flight

The true explanation of soaring flight is to be found, as is well known, in the two last hypotheses; that of the ascending currents and that of the irregularities and fluctuations of a horizontal current, that is to say, of the wind; to which hypotheses respectively correspond two forms of flight, known to-day, according to Kármán, as *statical* and *dynamical soaring flight*. It is, however, a matter of fact that up to the present man has succeeded only in making statical soaring flight, while with regard to birds the general rule is always statical soaring flight, while dynamical soaring flight is a particular feature limited to a restricted class of birds only. It is to the French scientists P. Idrac and A. Magnan[11] that we are indebted for having quite recently experimentally established that among birds only the albatrosses are able, in particular circumstances, to carry out dynamical soaring flight, by utilising the disturbed motion of the low layers of air produced by the motion of the sea waves, while, in general, birds soar by finding support on ascending currents.

Ascending currents, however, were quite lacking at Gross Lichterfelde, where the brothers Lilienthal experimented, and therefore soaring flight could not be accomplished. But Otto Lilienthal, who had not taken this into account, attempted to explain the failure of his efforts by an odd argument, which he formulated shortly before his death and which is mentioned by his brother Gustavus. Soaring flight, according to Otto Lilienthal, is the highest form of flight which was last reached by Nature, while gliding flight is the most elementary form of flight, the first step made by Nature in the evolution of flying creatures. Therefore, we cannot pass directly from gliding to soaring flight, but we must pass through the intermediate stage, that is through flapping flight.

It was thus that Otto Lilienthal in the last period of his life returned to flapping flight from which, like all at that time, he had previously started. For his flapping wing machine he had constructed two small carbonic acid engines to provide movement of the wings.

Side Lines of Aviation

To put forward criticisms against the aeroplane to-day, when the latter has reached such remarkable figures in speed, range, and ceiling, may appear somewhat strange. Nevertheless, it cannot be denied that the rigid wing of the aeroplane, while favourable from the point of view of advancing speed, owing to its lesser head resistance in comparison with a flexible wing, is unfavourable with respect to the latter from the point of view of safe landing, *i.e.*, at very low speeds, such as the popularisation of private aviation principally needs.

This problem could be solved by means of the so-called "variable surface wing," allowing of an increase in range between the maximum and minimum speed of the machine, but its practical realisation is faced by difficulties which are perhaps insurmountable, owing to the necessity of combining the mechanism involved with high strength and lightness.

There are two other methods of dealing with the problem of low speed landing; one consists in increasing by some devices the maximum lift coefficient

[10] "Der Vogelflug als Grundlage der Fliegekunst," by Otto Lilienthal 1890. Second edition, 1910, p. 73.

[11] P. Idrac. *Etudes Expérimentales sur le vol à voile.* Paris, 1922. *Contribution à l'étude du vol des albatros.* C.R.A.S. 7 juillet 1924, and *Le vol des albatros.* Rech et Inv. 1925.
A. Magnan. *Recherches Expérimentales sur le vol à voile.* Paris, 1922. *Le vol à voile.* Paris, 1925. *Le vol des oiseaux et le vol des avions.* Bulletin Technique n. 74 Juin 1931. Services Techniques de l'Aeronautique.

of the wings. Such devices, however, as the well-known Handley Page slotted wing, seem to Professor Th. von Kármán[12] to be more efficient in preventing stalled conditions than in ensuring a real advantage in landing speed. The other method consists in keeping a high *relative* speed between the air and the supporting surfaces, when the forward speed of the machine is lessening, by means of some suitable movements, whether turning or flapping, a method incorporated in the autogiro, the helicopter, and the ornithopter, called by Prof. Th. v. Kármán the sidelines of aviation.

In the autogiro the top speed is somewhat reduced in comparison with that of the aeroplane, but the reduction of the minimum speed is much greater in proportion. Safe landing is ensured with the autogiro. The helicopter has not yet passed the experimental stage, owing to its great difficulties of stability. The realisation of the ornithopter is problematical, principally on account of the very high accelerations which must be imparted to the wings (about 100g.). Imitation of Nature, which in the matter of the ornithopter has tempted so many people, is not always possible, owing to the different working conditions between raw materials and living tissues.

Nor must it be forgotten that Nature herself almost totally renounced flapping flight for big birds, which have recourse to it for short periods only. They are extremely fatigued by it, and, when prevented by atmospheric conditions from soaring, are constrained to alight.

The Old Explanation of Sustentation in Flight

Now we can begin to answer Mr. R. A. Southwell's[13] philosophical question: "*Why, exactly, does an aeroplane fly?*"

Before setting forth the modern theory let us recall the preceding one, the main feature of which was that of demonstrating that the flight of a flying machine was *in practice* impossible.

The celebrated astronomer Lalande calculated that a man, to be able to sustain himself in air by means of a flapping wing apparatus, would be obliged to beat two wings of a total surface of 1,367 sq. ft., while another distinguished mathematician, Navier, calculated that a swallow needs 25 h.p. to fly, thus arriving at the conclusion that the muscular energy of birds was enormous in comparison with that of man.

Later, when aeroplanes flew, there was considerable amusement over the results obtained by these mathematicians, and mathematicians and mathematics were looked upon with a certain amount of suspicion. But mathematics is like a mill which gives meal in conformity with the corn which has been put in—put in an erroneous hypothesis and you will obtain erroneous results.

Now the hypothesis accepted by the scientists of a century ago as the basis for their calculation, was an hypothesis on the behaviour of fluids against an obstacle. It was recognised as not being strictly true, but was usually employed, for want of a better, to calculate the resistance of fluids. The scientists had this on their side, in accepting the hypothesis, that for calculating the water pressure on dykes, on the bows of ships, on mill wheels, on the pillars of bridges, etc., it supplied figures sufficiently in agreement with those obtained by experience. This agreement between theory and practice held good in all cases, like these, in which the fluid met the obstacle at an angle between 50° and 90°. But it was not applicable to small angles. A wing meets the air at small angles and from this came the erroneous results.

The hypothesis consisted in supposing that, when a wing (or in general a body of whatever form) moves through air, it strikes against the air particles

[12] "Die Seitenwege der Luftfahrt," by Th. v. Kármán. Z.F.M. n. 16, 1931.

[13] "*Aeronautical Progress, 1914 to 1930*," by R. A. Southwell. Aircraft Engineering, June, 1930, p. 139.

found on its path, which then rebound to a greater or less extent, according to their being more or less elastic. This hypothesis became known as the Newtonian hypothesis, although Newton had used it only for an imaginary fluid, characterised precisely by this hypothesis and, as Col. B. De Villamil so well remarked,[14] he would never have extended it to real fluids—a fact generally unknown to students who, therefore, have held Newton responsible for the demonstration, given on the basis of the said hypothesis, that human flight was impossible!

Apart from this erroneous hypothesis, the argument followed by mathematicians of a hundred years ago was quite right, since it rested on the fundamental principles of mechanics, in particular on the law of action and reaction being equal and opposite, and of communication of momentum. In fact they said that, for it to be possible for a wing to be supported on air, it was necessary that the push downwards by the wing on the air should be equal to the counter-push upwards by the air on the wings, the value of which is exactly measured by the momentum communicated per second by the wing to the air.

Now, owing to the hypothesis made on the behaviour of the air with regard to the wing, the result was that the mass of air set in motion by the wing was very small, so that to sustain even a light weight it was necessary to have either an enormous wing area to move air enough, such as that calculated by Lalande, or a very great energy of motion, such as that calculated for the swallow by Navier. The conclusion in short was this, mechanical flight is an illusion.

The Modern Theory of Sustentation in Flight

To establish an exact theory of sustentation in flight it was necessary to change the hypothesis on the motion of air in the vicinity of the wing. This substitution was made by Mr. F. W. Lanchester who, in his Birmingham lecture of the year 1894 already referred to, gave a first outline of his theory, which later, in the years 1907 and 1915, he completed, without however altering its essential lines.[15]

Lanchester discovered, in fact, how the motion produced in the air by the passage of the wing is much more extended than that admitted in the previous theory, and the mass of air involved was much greater, owing to the fact that when the wing advances through air the particles of the latter, which are found in the path of the wing, do not strike it singularly, but deviate, putting in motion all the neighbouring layers of air.

Let us see how this motion happens. The wing advancing at an angle through air exerts on the latter a pressure on its lower surface, and allows it to flow freely on its upper surface. Masses of air are thereby caused to circulate from the under to the upper side of the wing, round its leading edge. It must not be understood that each particle of air makes a complete circuit round the wing, since the latter rapidly advances and leaves the air particles behind.

The phenomenon connected with circulation is the creation of a difference of pressure between the lower and upper surface of the wing. On the lower surface an increase of pressure takes place in the air, while above the wing there is a reduction of pressure, *i.e.*, a suction. The result is that the wing experiences an upward force or lift.

There is another important phenomenon to be considered in the circulation theory. On account of the inequality of the air pressure between the lower and upper surface of the wing, a second circulation of air is produced (of the same strength of the former), owing to the fact that air tends to flow from the pressure region to the suction one, *i.e.*, from the lower to the upper surface of the

[14] "*The Sin² Law*," by Col. R. De Villamil. Aeronautics, Feb. 1913, pp. 55-56.
[15] F. W. Lanchester. "*Aerodynamics*," London, 1907; "*The Flying Machine: the Aerofoil in the Light of Theory and Experiment*," London, 1915.

wing, round its lateral tips. While this second circulation is taking place, the wing advances. The result of the superimposing of the circulatory motion of air round the wing tips on the translatory motion of the machine is that from both the wing tips two air rotating " ropes " are thrown off, in the form of a wake, and inclining downward.

The two masses of air in whirling motion which the wing trails behind it have been called trailing or tip vortices; the mass of air circulating round the wing has been called vortex on the wing.

In conclusion when an aeroplane flies its wings cause in their neighbourhood a vortex motion in the air which ends with the movement of a mass of the air downward.

The sustentation of the aeroplane must therefore be considered as the *reaction* of air to a downward *action* carried out on the air by the wing, while the momentum communicated per second by the wing to the air is exactly the equivalent of the load sustained.

The Lanchester-Prandtl Aerofoil Theory

It is to the credit of Lanchester, that by his *discovery* of vortex motion, he has harmonised the phenomenon of sustentation in flight with Newton's Mechanics, that is, explained it by the fundamental laws of action and reaction and of the communication of momentum.

Lanchester's contribution to the foundation of the science of aviation, however, does not end here, although for the present we will no longer insist on it, except to say that his work has not yet been fully understood by all aerodynamicians, who, being accustomed to proceed along the lines of mathematical formulas, find some difficulty in grasping his profound physical insight, which lies under his plain English, divested (in his own words) of all mathematical ornament.

Lanchester's *physical* explanation of the phenomenon of sustentation in flight was followed by its *mathematical* determination, which we owe to Professor W. M. Kutta[16] (who arrived independently to the idea of circulation) and to N. Joukowsky,[17] a determination which was then completed by L. Prandtl, to whom we are indebted for the complete mathematical treatment of the wing theory.[18] In recognition of the contribution made to this theory, on different and independent lines, by Lanchester and Prandtl, the theory, according to a proposal of Mr. H. M. Martin,[19] is known to-day under the name of the Lanchester-Prandtl aerofoil theory.

A special contribution by Prandtl to this theory is that of having explained the mechanism by which air, under the pressure of the wing, is caused to circulate round the latter, thus ultimately providing for its sustentation. The mechanism is found in the friction of the air on the wing, by which friction an initial vortex is shed at the trailing edge of the wing, such vortex constituting the starting point of the circulation.

Friction of the air on the wing (viscosity) is thus a necessary condition for sustentation in air, as also for propulsion. Aerial locomotion depends ultimately on viscosity, but viscosity makes impossible any complete mathematical treatment of the problem. It is to the credit of Prandtl that he has given us a picture

[16] W. M. Kutta. " *Auftriebs Kräfte in strömenden Flüssigkeiten* " (Illustrierte Aeronautische Mitteilungen, 1902, p. 133). Other papers by Prof. Kutta on the matter followed in the years 1910 and 1911.

[17] A first paper in Russian, entitled " *On Adjoint Vortices,*" was published in the year 1906. Others followed in the years 1906, 1910, 1911.

[18] " The fundamental work of Prof. Prandtl on the aerofoil theory was published in the years 1918 and 1919 with the title " *Tragflügel Theorie* " (Göttingen Nachrichten).

[19] " *The Elements of the Lanchester-Prandtl Theory of the Aeroplane Lift and Drag* " (Engineering, 1924).

of the phenomenon which, while in agreement with our experience for fluids like water and air, introduces simplicity into our calculations. The first contribution by Prandtl to the problem goes back to the year 1904, with his paper on the motion of a fluid with very small viscosity[20] in which he laid down the bases of the well-known "*boundary layer*" theory. This was the famous paper which started a whole series of theoretical and experimental investigations on the problems of turbulence and viscosity: problems towards which at the present moment general attention is concentrated in all countries.

The Possibility of Mechanical Flight According to Leonardo da Vinci

It is not possible to close this historical survey without recalling the work of Leonardo da Vinci on mechanical flight, a reconstruction of which was attempted four years ago, on the occasion of the London International Aero Exhibition in the Italian Section of the Royal Aeronautical Society, with various models, drawings and photos. Some of these were afterwards presented by the Italian Air Ministry to the Science Museum, Kensington, where they are on view.

Without repeating here the details of that reconstruction (an account can be found in a recent publication),[21] we will limit ourselves to establish only some fundamental points of the conception of flight by Leonardo da Vinci. From them it will appear how, four centuries ago, at a time when flight was still considered more a supernatural than a natural feature, the genius of Leonardo da Vinci explained the sustentation of birds in flight on quite physical hypotheses, therefrom deducing the mechanical possibility of flight.

Leonardo based his explanation on the hypothesis that air under the stroke of the wing would compress, thus becoming capable of resisting and supporting the wing. The hypothesis that air should operate in flight by its compressibility was indeed quite a reasonable idea, since compressibility is the most apparent mechanical property of air, and even to-day the uninitiated largely attribute the phenomenon of sustentation in flight to its agency. In fact, they, like the great investigator of old, are unaware that the density of air can be regarded as constant for all velocities up to a very high limit, that of the velocity of sound.

The question then asked by Leonardo was the following: What must the stroke of the wing be for air to acquire the compression necessary to support the bird? And his answer was that the wing stroke has to be made with a velocity greater than is the velocity with which the pressure can be transmitted from the layer of air directly struck by the wing to the back layers of air, as, according to him, in these conditions, air not being able to yield to the too rapid pressure, loses its gaseous properties, becoming locally and almost instantaneously as a solid body.

The condition for compression of the air, that is for sustentation in flight, was therefore a wing stroke of sufficient rapidity. But, according to Leonardo, it was not necessary for still air to be struck by a flapping wing, since everything would remain unaltered if a motionless wing were struck by air in motion, that is to say, by wind. In short, he recognised that the essential condition for the realisation of flight is the existence of a *relative* velocity, between air and wing, of sufficient intensity.

A further step made by Leonardo in this line of thought was the statement that flapping flight and soaring flight came thereby to depend on one principle only, with the result that the mechanical realisation of flight, which he after long meditations had been obliged to give up by the method of "*the beating of*

[20] "Uber Flüssigkeitsbewegungen bei sehr Kleiner Reibung." A paper presented to the Mathematical Congress of Heidelberg (1904), and published in the year 1905.

[21] "*I modelli delle macchine volanti di Leonardo da Vinci.*" Rome, "L'Ingegnere," n. 2, 1931.

wings," appeared to him possible, instead, by the method of "*the favour of the wind.*"

Leonardo arrived at a clear idea of the possibility of solving the problem of mechanical flight on the lines of soaring flight in the year 1505[22] in Florence, conceiving the hope of being able to start his first flight from the top of a hill (Monte Ceceri, 1,370ft.), in the neighbourhood of which he had many times observed the soaring of birds of prey.

The Glorification in Leonardo da Vinci of Human Flight

At the feet of the Ceceri hill, on the road from Florence to Fiesole, twenty years ago there was placed a memorial tablet on which two passages are engraved, taken from his manuscript treatise "*On the Flight of Birds*" of the same year 1505, in which Leonardo announces with enthusiastic words his great first flight, which in his excited fancy he seemed to see already realised. And on the top of this hill, overlooking the whole city of Florence and sacred to Leonardo's dream of flight, we may hope to see one day erected a great monument in his honour.

[22] For further information on this point see "*The Aerodynamics of Leonardo da Vinci.*" "The Journal of the Royal Aeronautical Society," n. 240, Vol. XXXIV., Dec. 1930.

CORRESPONDENCE

Slots in the Wings of Birds

The Editor, THE JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY.

Dear Sir,—Dr. Lachmann in the article, entitled " Slots in the Wings of Birds," which appeared in the April number of the JOURNAL OF THE ROYAL AERONAUTICAL SOCIETY, gives some very interesting and instructive comparisons of the loadings of birds and aeroplanes, and at the same time makes some comments on the subject of slots, which seem to call for an answer.

Regarding the wrist slot, he writes, " I am firmly convinced that this is quite a useless appendage. . . . I cannot believe that it has any aerodynamic importance, because it is much too small and far too much inboard to be of stabilising assistance." He bases this conviction on figures which he gives for the size of the wrist slot, namely, " hardly 5-8 per cent. of the semi-span."

It would appear either that some mistake was made in arriving at these figures from the data provided in the original paper, entitled " Safety Devices in the Wings of Birds," or that they were obtained independently by some different form of measurement. According to the table of flight characteristics in the paper, the wrist slots of birds vary in length between 10-26 per cent. of the semi-span. Actually in the table, the slots are given as fractions of the length of the wing. This was considered to be a fairer method because that part of the bird's body which must be included in the semi-span does not seem to have much to do with the matter. Considered in this way, the slots come out as 11-30 per cent. (slot factors 0.11-0.30); in other words, the smallest is more than twice as big as Dr. Lachmann says, and the largest more than four times as big.

The photograph of the wing of a willow grouse, a continental variety of our Scottish ptarmigan, shows that in this bird, at any rate, the wrist slot is of no mean size; the wing is 11in. long and the slot 2.7in. It is hard to believe that a slot of this size is useless, especially when the relatively small slots of more easily observed species, such as the rook and seagulls, can clearly be seen operating when the birds are gliding at slow speed. Many photographs bear out this observation.

With regard to position in the wing, no claim has been made by the writer that the wrist slot may be of stabilising assistance. It is thought that it acts for the most part as a lift slot, in which case the centre of the span of the wing would surely be a good position for it.

The case for the utility of the slot is strengthened by the fact that young birds, on leaving the nest, apparently have fully developed bastard wings, while their main wings are still considerably smaller than in the adult bird. So far, fledgling birds of only three species have been measured, but they all show this significant discrepancy in the size of the wrist slot compared with the full grown

birds. As can be seen in the table of flight characteristics, a fledgling swallow has a wrist slot which is 17 per cent. of the length of the wing, and the adult bird only 13 per cent. The figure for a fledgling song-thrush on its first day out of the nest was 30 per cent.; for the full-grown bird 17 per cent. It is pleasant to think that these young hopefuls, who have to set out on their first solo flights without any dual instruction, perhaps have their chance of success measurably improved by the provision of man's size safety devices in their untried and under-sized wings.

This leads to Dr. Lachmann's firm belief that the slots of birds do not act as safety devices in the same way as those of aeroplanes, and that the senses of birds are so marvellously developed that they have no need of safety devices.

Under-surface of the right wing of a willow grouse with the slot forming bastard wing raised in order to show its proportions.

This particular comment perhaps does not call for an answer, for it probably hinges upon different interpretations of the words "safety device," but it does bring out a rather interesting point in connection with safety in flight. It is common knowledge nowadays that many good pilots, whose senses must have become somewhat attuned to their calling, persist, in spite of warnings from the Air Ministry, in using the slots in their machines more for reducing the minimum speed at which they can fly with safety than for providing a margin of error. It is the same with the comparatively small winged birds which cannot, on account of their manner of living, afford to have long lightly loaded wings of the seagull-swallow type; they unconsciously rely upon their slots to permit them to fly at lower speeds, and therefore with greater safety, than they could manage with unslotted wings. Their marvellous senses would be of no avail without the necessary gear for carrying out their messages. Considered in this way, surely the slots in birds' wings act in the same manner as those of aeroplanes.

Dr. Lachmann asks, "Why, if slots in birds' wings are safety devices, do gulls not have them?" This question calls for another, "Why, if slots in aeroplanes are a safety device, do some machines not have them?" The answer to both questions is that there are certain types of aeroplanes and birds which can be provided, on account of the purpose for which they were designed, or their manner of living, with wings of such a shape and size that even at low

speeds they will do all that is required of them without the use of angles of incidence great enough to warrant the provision of slots.

The advantages which slots give to birds in gliding flight are only a secondary, though highly interesting consideration, and Dr. Lachmann's conviction that they are of first importance in flapping flight is heartily agreed with.

Yours faithfully,

R. R. GRAHAM,

Lieut.-Commander, R.N.

REVIEW

Sailplanes

C. H. Latimer Needham, M.Sc. (Eng.) Lond., F.R.Ae.S. Chapman and Hall.

This book deals with modern practice in the design and construction of sailplanes and contains a great deal of useful information on this subject. It contains chapters dealing with design, constructional details, stress calculations, and pilotage, and concludes with a useful appendix containing standard data and certain British Gliding Association Regulations.

The British Gliding Association Regulations for Airworthiness are explained in detail, and it is interesting to note the difference between these requirements and those of certain other countries. In several cases this difference is serious. For instance, the German figure for the load on the rudder for stress purposes is 30 lbs. per square foot, while our own averages about $7\frac{1}{2}$ lbs. per square foot. Knowing the great care with which these matters are studied in Germany, and bearing in mind that their experience in such matters is greatly superior to that of any other country, the matter should not be left where it is. Either the Germans are wrong or we are, and if we are in error our sailplanes cannot be safe structurally.

It certainly seems that the time has come when steps should be taken to standardise these airworthiness requirements for all countries. International contests should certainly be a feature of sailplane meetings in the future, and there is bound to be difficulty in managing such meetings unless the machines are built to the same requirements. There should be no possibility of a competitor's machine obtaining an advantage in performance at the expense of structural safety.

The author deals to some extent with the methods of stressing the various parts of sailplanes. To deal with this matter properly would take easily the whole of the book, and it is certainly doubtful whether an inadequate discussion, such as is given here, is worth doing. Certain matters of considerable importance are not referred to at all. For instance, in the discussion concerning air loads on main planes the air load is taken as proportional to the chord, allowance being made for tip effect. No allowance is made for aerodynamical or geometrical twist, and it is precisely in high aspect ratio gliders that the redistribution of air loads due to this factor might be expected to be of serious importance. Again, it is noted that it is suggested that the nose of the wing when covered with three-ply wood to the main spar may be treated as a circular tube for torsional design purposes. This is a most doubtful assumption.

All the chapters on stress matters suffer from a looseness of discussion which is most undesirable in a book of this character. As all such matters are well and adequately dealt with in the Air Ministry Hand Book of Strength Calculation, it is difficult to see why such matters need be discussed in detail at all in a book of this character, except as far as the load factors, and certain of the loads may differ

from those found in power-driven aircraft. A reference to the Air Ministry publication should be all that is necessary.

It would have been of great interest if the resistance curves for some of the better-known sailplanes had been given. It would be difficult to ascertain from the information given in this book the precise form of the resistance curve for any new machine and what modifications to this curve would arise from the various possible positions of the wing relative to the fuselage. It would, I think, have improved the book considerably if the section dealing with stressing were cut out and the space available devoted to such matters as those last referred to.

The book gives a clear idea of the present state of sailplane design, and the chapters on piloting, launching, etc., and on existing types of sailplanes contain much useful information.